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THE STATUS AND FUTURE OF THE AMERICAN AGRONOMIST[1]

ON the occasion of this fourth annual meeting of the American Society of Agronomy it is of interest to note that our membership has now grown to more than two hundred and that our published proceedings are finding their place not only in the private libraries of American agronomists, but also on the shelves of the libraries of the leading colleges and universities of the country. Indeed the time seems to have arrived when this society should seriously consider supporting a journal. We have definitely put our hands to the plow. It behooves us, therefore, to be diligent, to push this society into the front rank of the scientific societies of the land, and to guard jealously against any and all influences which may interfere with the highest development of its individual members and thereby restrict its opportunity for public usefulness.

It must be recognized that no scientific body can be brought to its highest plane nor be made of the greatest service to our American people unless the ideals of its individual members are high. The future of agronomy in this country is, however, not only dependent upon such ideals, but also, in a very great degree, upon the administrative attitude of the institutions which we serve.

To the professor who, a generation ago, was covering in his way the whole range of agricultural science, the field of the present-day agronomist may seem narrow; but those who have kept pace with the march

[1] Presidential address before the American Society of Agronomy, November 13, 1911.

of recent events must be impressed with its breadth and by the fact that even greater specialization is foreshadowed in the near future, when the subject of agronomy may readily resolve itself into several distinct fields of effort.

The student of farm crops can no longer be content with a knowledge of what belongs to the art of crop production, but must now be well grounded in systematic botany, especially in its relation to the bacteria and fungi, and to the plant families which embrace the weeds, grasses and the common farm crops. He should understand and follow the work in breeding which is being done throughout the world.

In order to deal with many of the problems with which he will be confronted as an investigator and which he should be able to fully grasp as a teacher, fundamental training in physiological botany becomes essential. Indeed, this is only the beginning, for the agronomist has not only to deal in detail with the plants which contribute directly to the food supply of man and of our domestic animals, but also with an extensive soil flora almost undreamed of a half century ago, upon the study and control of which, for the furtherance of agriculture, the world is to-day barely entering. The agronomist of the future must not only deal with the effect of these soil plants upon each other and upon the higher plants in their parasitical and symbiotic relations, but also as producers of ammonia and nitrates, and as destroyers of compounds of sulfur and of nitrogen within the soil.

As suggested by the recent investigations of soil amebæ by Hall and his coworkers at Rothamsted, he must also deal with microscopic animal denizens of the soil which may militate against, or, as perhaps may yet be found, aid in the growth of certain beneficial fungi, and other microscopic flora. In fact, the end is not yet, for chemistry now plays its rôle in furnishing the agronomist carbon bisulfide, and other substances for combating unfavorable animal life in the soil. Chemistry also plays its part in controlling and regulating the chemical reaction, and hence the dominance or decadence of various types or even of individual representatives of the soil flora.

There is reason to believe that we are to-day but entering upon the study of the organisms and of the conditions best suited to ensure the assimilation of atmospheric nitrogen by non-symbiotic means.

The whole question of the use of fertilizers and of their action is daily becoming more complex. It was a simple proposition when one supposed that it was merely essential to learn what elements crops removed from the soil and then to supply a proper part thereof, without special reference to the particular compounds used to supply them. To-day, cognizance must be taken of the effect of the associated compounds. The sulfuric acid and chlorin combined with ammonia in ammonium sulfate and ammonium chlorid may have a highly toxic effect from the outset, or such effects may soon develop in certain soils if care is not taken to maintain a proper basic condition. The subsequent effect of organic nitrogenous manures is quite different on some soils from that of nitrate of soda. Even though the avoidance of chlorin and sulfuric acid, when combined with ammonia, is of vital importance under certain circumstances, it is often less necessary under the same conditions if they are in combination with potassium, calcium and magnesium. For still other crops, or on another soil, they may nevertheless be used with good effect.

Another illustration is afforded by nitrate of soda. The residual effect of re-

peated annual applications may result in the most marked soil improvement, rendering successful the cultivation of a whole series of crops where they could not be grown successively before. The same treatment, even for a long series of years, may still fail to correct the existing soil conditions enough for other groups of plants. The continued use of nitrate of soda on another soil may cause it to become puddled until it is rendered practically unfit to be a habitat for most agricultural plants. For certain plants, such as the radish and beet, the residual sodium from nitrate of soda may perform valuable physiological functions which would be lacking, or of slight importance, in connection with certain other plants. Raw rock phosphate may be valuable as a fertilizer on the black soils of the Illinois corn-belt and for crops usually grown there, but for the light sandy soils of the Atlantic coast and for certain trucking crops its use at prevailing prices could perhaps not be recommended. To add to this complexity certain text-books proscribe the use of lime with superphosphates, or on soils where undissolved phosphates are to be used, and yet there may be soils on which liming is essential to the most economical use of each. It is, in fact, not enough that the agronomist should bear in mind and master all of these details, but now he is called upon to consider the specific requirements for lime and other substances, of hundreds of varieties of plants. He must also consider the alleged toxic root excreta and methods for rendering them innocuous, and he must take cognizance of the catalytic action of manganese and other elements not heretofore grouped in the galaxy of fertilizers and soil amendments. He must now consider the effect of legumes and other plants upon those growing in association with them and the effect of given crops upon those which follow. The whole question of maintaining conditions favorable to nitrification is of prime importance in certain sections of the United States, and in this connection chemistry is again the handmaid of agronomy; yet in certain of the semi-arid regions of the middle west excessive nitrification is said to have become a scourge which is wiping out many of the most promising orchard industries. It must be evident that the agronomist must therefore be something of a climatologist, for in certain of these features the weather conditions are the chief governing factors.

The successful agronomist must also deal effectively with a host of plant parasites which may attack the roots, the base of the stems, or the other aerial parts of the plants. Some of these may be killed by poisons, whereas others can not. Even the sucking and boring insects furnish a problem in themselves, long after the entomologist has determined the essential features of their life history. Just as "every animal has its fleas and these have fleas to bite 'em," so the plants have their many animal and fungus parasites, with which the agronomist is forced to deal.

Since the soil is one of the chief concerns of the agronomist, and it is known to be teeming with many forms of microscopic life of beneficial or injurious character, it is important to take cognizance of the possible effect upon this life of the various kinds of organic matter and of fertilizers which may be introduced into the soil from time to time.

Notwithstanding the recent assertion that practically the same minerals are found in all soils, that plants feed from very weak solutions, and that the soil solution is being continually renewed, we cannot complacently fold our arms and watch the workings of the divine providence in

the production of food for the human race; for some soils appear still to lack enough available plant food at certain stages of growth, and others give rise to conditions, naturally, which require chemical and physical amelioration. It is an incontrovertible fact that soils derived from given kinds of rocks have usually distinct needs, whereas such treatment may be wholly neglected in the case of soils derived from rocks of a different character. For these and other obvious reasons the agronomist, in order to be well equipped to meet situations which may arise in another state, or in a new position to which he may be called, will find it of distinct aid if his fundamental educational equipment includes geology, mineralogy and physics in its special application to the many problems of the soil.

The agronomist will be brought face to face with emergencies and questions involving physical chemistry, the foundation for which is supplied not only by general chemistry, but also by knowledge of mathematics involving the calculus.

Finally, above and before all should be placed the subject of English, the call for which in some station bulletins is obvious, and in the use of which none can be too proficient.

It may be argued that the fundamental educational requirements as presented encroach upon other domains of science, that they are too comprehensive and are more exacting than the conditions demand. Nevertheless our progress as agronomists can not attain its maximum by depending wholly upon men who are trained only in a narrow specialty. Those engaged in given lines of agronomical research must have a sufficiently broad training in order to grasp the significance and bearing of factors lying frequently much outside of their strict domain. Had not Hellriegel possessed an outlook broader than that circumscribed by the mere limits of chemistry, it is problematical if the discovery of nitrogen assimilation through the intervention of microorganisms might not have remained a problem for ourselves.

It is not enough that the teacher or investigator in agronomy be skilled in its art, but he must be trained in all of the natural sciences which are closely related to crops, fertilizers, soil amendments and to soils themselves in all of their several relations. The man who looks forward to service in the west or middle west can not neglect the chemistry of fertilizers in their relation to the special crop and to the special soil, for the fertilizer problem is advancing westward at a rapid rate and many of the present-day needs of the east will, in the near future, become the needs of much of the west and middle west.

From what has been said it must be obvious that the ordinary college course can not be considered an adequate preparation for the life work of the agronomist, whether he be engaged in teaching or in research, but that this must be supplemented by at least three years' work at the university. Conversely, it must be equally obvious that he who would succeed in the fullest degree in his scientific achievements in the domain of agronomy must also be familiar with the subject as an art, and if this knowledge was not acquired at home on the farm, before the beginning of the college course, it should surely be made a part of his equipment before entering upon the university course.

The demands of the times make imperative not only a knowledge of the art of agronomy and the possession of the foundation contributed by the college and university, but they demand that the teacher or investigator keep continually in touch with the work of others in this and other

countries, and to this end a reading knowledge of French and German, and if possible of other foreign languages, is essential. The argument that it is sufficient to merely read the abstracts of papers is specious. The investigator should never be content with anything short of the original, since ideas as to the relative importance of the different parts of an investigation are often widely variable, dependent upon the outlook or particular experience of the abstractor. It, therefore, not infrequently happens that a point which may be passed over as insignificant is vital to the work of some investigator, who, if confined solely to consulting the abstract, might never be able to profit by it.

It is obvious that the teacher must have sufficient time at command for daily recreation if he expects to maintain himself in condition to present his subject matter year after year to his classes in a clear and forceful manner. The same thing is necessary for the investigator in order that he may be keen and alert in the pursuit of his problems. He is then in condition to recognize points of attack which the man pressed and wearied with many duties might pass by unnoticed. To him who would be a strong, full man, capable of imparting inspiration to his students or of attacking problems of research with the true enthusiasm which is essential to success, time must not only be allowed for renewal of physical strength and for abundant reading, but *also for undisturbed and consecutive thought.* This means that no institution can long expect to be a leader in the field of education or in research, if its policy is to demand so much by way of other duties or so many hours of teaching that its employees can become leaders neither in thought nor in research in their chosen specialties. A university president, in a recent address, announced that those teaching at his institution were hereafter to be measured for their fitness by their output in research. Such a policy, while prompted by a commendable spirit, might be more nearly applicable in a new institution in which the teaching demands are reasonable, but it is likely to work the grossest injustice if applied immediately in a college where worthy professors have grown old in a treadmill of exacting service, which has left no time for gathering inspiration nor for work of research. Such men, if given the opportunity at the right time, might have won a national or world-wide reputation as investigators, for they may have been original, diligent and fired with an enthusiasm which the institution itself gradually smothered and snuffed out. Such men should not be cast aside like an exhausted sponge, for the institution and the state owe them a debt which they can not repay. Again, a college professor can not always do his best work if made to feel that his tenure of office depends upon his yearly output in research. Such avowed watchfulness by a president or by a committee on efficiency is likely to lead to superficiality, to hasty publication, or to create unrest disastrous to research of a high order and to bring many disastrous consequences in its train.

Object lessons of spoiled investigators are especially common, in many of the smaller colleges, and even in many of the larger ones; yet the time may never come when it will be safe to measure the fitness of all men for college teaching solely, or even chiefly, by their research output. Nevertheless, one can not but recognize the desirability of encouraging teachers to practise exhaustive reading on special subjects, or to undertake special advanced research, whenever the demands of their positions and the attendant circumstances render it possible.

From what has been said it would appear that all teachers, and those who are selected to conduct research, should have at least three years of university training superimposed upon the college foundation. In saying this the writer recognizes that some of the best men in the country have not had this experience, but yet have won an enviable reputation in their respective lines, even in certain cases outstripping many who have enjoyed a more extensive fundamental training. It must, nevertheless, be recognized that such men have succeeded not in consequence of their handicap, but in spite of it! They were close observers, diligent students, and were possessed of original and judicial minds.

Admitting that the university training is a great desideratum in all cases, the problem presents itself of lending sufficient encouragement to young men so that they will be willing to devote three of the best years of their lives, and a large sum of money, to university study.

At almost every session of the Association of American Agricultural Colleges and Experiment Stations some college or university president or station director has bemoaned the difficulty of finding adequately trained men to fill the higher positions, especially in research. Indeed, the Secretary of Agriculture, the Hon. James Wilson, has repeatedly stated in public addresses that the Department of Agriculture finds it impossible to secure in this country men adequately qualified for many of the positions in the federal service, on which account his department is forced to train its own men. This leads to the query: Why does not the same principle of supply and demand hold as in lines of industry? It is a fact, which I think will be disputed by none who are well informed, that this country furnishes exceptional opportunities to-day for the young man just out of college. Perhaps, indeed, if some of them, like men known to the writer, were forced to begin, after completing a four-year college course, at a salary of from $16 to $20 per month, all of which was required for board and clothes, they might be willing to make greater sacrifices than at present in order to insure for themselves a future, by securing a university training at whatever cost. To-day, however, the young graduate can readily command an initial salary of from $800 to $1,200, and many have been advanced within from two to three years to salaries as great as, or greater than, those paid in other reputable colleges to much older and more experienced men who have enjoyed a university training.

When these young men look about them in the institutions with which they are connected they may even find others at the heads of departments who have never enjoyed graduate study. They may also find those who have made the sacrifice, struggling by all sorts of means to add enough to their insufficient incomes to enable them to support a small family, with few comforts, no luxuries, and even with deprivation and need before them, in case of unusual illness or misfortune. It is no wonder, under such circumstances, that he thinks "a bird in the hand is worth two in the bush" and prefers to go on accumulating, rather than to spend three years' time and the savings of other years in order to secure the mere intellectual advantage of further study. As I have several times pointed out in public addresses, there can be no permanent remedy for such a condition short of an assured pension for those who have given ten to fifteen years of efficient, faithful service to such colleges and stations, or there must be occasional half-year intervals of freedom and a marked and progressive increase in compensation for the older and experi-

enced men. As concerns pensions, one that does not become assured until the end of a thirty-year period of service, while a great boon to those who finally receive it and a welcome aid to the president in unloading undesired or superannuated professors, nevertheless fails to furnish that assurance of security in case of disability or later financial difficulties which encourages the professor to satisfactorily equip his library, to travel, to study and to surround himself by the broadening influences which are essential to his greatest intellectual development and to his greatest usefulness to the students who come under his instruction. In this matter of pensions and conditions surrounding them we have a valuable lesson to learn from Germany.

It has been argued by some that the early assurance of a pension robs the prospective recipient of initiative and enthusiasm in his chosen profession and encourages a letting up of his intellectual activities. To such as advance this argument the writer begs to enter an emphatic denial of the justness of the accusation, for from his personal acquaintance with professors in many of the leading German universities and his observation of their spirit of research, he is convinced of the utter incorrectness of such a position. Indeed, nowhere in the world could one find greater devotion to duty, greater willingness to make personal sacrifices, or greater zeal in investigation, than among the professors of these German universities, who can look forward complacently to the future if disabled, and in any event with the comfort and knowledge that their families, after their work is done, will be cared for properly as a reward for a lifetime of faithful public service.

Finally, this society will do well to encourage the development in our universities of higher and broader graduate courses in the applied sciences related to agriculture. Let us use our influence as a body to secure from the Carnegie Foundation, for the teacher and investigator in the smaller land-grant colleges, the same fair and just recognition for quality and amount of public service rendered as is accorded to the teacher of mathematics or of the classics in the older classical colleges of the country. If necessary, let the American Society of Agronomy urge upon congress the provision of a pension system for the land-grant college, based upon a reasonable probationary limit of service as a condition for its becoming assured. If to this these colleges will add the sabbatical year, or will allow a full half-year in every five, and will give adequate and progressive advances in salary with the years of service, we shall soon see plenty of young men fitting themselves well for the work of teaching and research.

In closing I would not fail to emphasize that young men entering our profession should do so with the missionary spirit and with the desire to serve their fellows uppermost in mind, but the situation to-day is such that many who set out with courage are forced, out of justice to their families and through failure to secure the reasonable comforts and necessities of life, to seek, against their will, such financial returns in other callings as are rarely the reward of the agricultural teacher and investigator.

H. J. WHEELER

THE INTRODUCTION OF PHYSICAL CHEMICAL CONCEPTIONS IN THE EARLY STAGES OF THE TEACHING OF CHEMISTRY[1]

THE question I have been asked to discuss is not a new one, but is, in my opinion, one of fundamental importance. Whenever any

[1] Paper read before the American Chemical Society in Washington, December 27, 1911.

great advance has been made in any branch of science, the question has arisen how early should this be incorporated in the teaching of that science; in a word, how closely teaching should follow research, and various answers have been given.

That we are dealing here with a fundamental question is obvious after a moment's reflection. Shall we teach the beginner, in a judicious way, of course, the science as it is at the time in question, or shall we teach him what is not only hopelessly out of date, but what is known to be absolutely untrue?

In answering this question we must take into account that the beginner of to-day is the advanced student of to-morrow, and the chemist of the near future. It is true that most of the beginners in any branch of science never pursue that science at any length, and to these perhaps the least harm is done by teaching the science in an out of date manner; but the question becomes more serious when we are dealing with those who propose to devote their lives to the branch of science in question.

Why has the question that we are discussing arisen at this time? As is well known, it has come to the front as the result of certain fundamental discoveries made in chemistry towards the later part of the last century. These are usually known as *physical-chemical generalizations,* because they were reached through the application of physical methods to chemical problems.

I think the term "physical-chemical" is unfortunate, because it may leave the impression that we are dealing here with something different from chemical, while, in fact, we are not. Indeed, I think the term "physical chemistry" is unfortunate, since it may lead to the conclusion that here is something that is not chemistry, while it is simply an integral part of chemistry. I greatly prefer the term "general chemistry" or "generalized chemistry"; since the generalizations which have been reached in this field concern most vitally and fundamentally the whole science of chemistry. This same thought is echoed in the title of Ostwald's great work, "Lehrbuch der allgemeinen Chemie." The term "physical chemistry" is, however, so widely disseminated, and the leading journals in this field in German and French both bear this title, so that the hope of reform in this nomenclature seems remote.

The generalizations that we have in mind are: The discovery of the Law of Mass Action, by the Norwegian physicist, Guldberg, and the Norwegian chemist, his son-in-law, Waage, in 1867; the discovery in 1886 of the applicability of the laws of gas-pressure to the osmotic pressure of dilute solutions of non-electrolytes, by one of the greatest men of science who has ever lived, Van't Hoff; and the explanation by Arrhenius in the same year, of the apparent discrepancies presented by electrolytes, *i. e.,* the announcement of the theory of electrolytic dissociation; of less importance perhaps is the interpretation of chemical valence in terms of Faraday's law, but scarcely so, at least from the pedagogical standpoint; and finally, the discovery of the electron, by Sir J. J. Thomson, and the instability of the chemical atom, by Rutherford.

The question then is, shall these generalizations be taken into account in the early stages of the teaching of chemistry, or shall they not? I know of no productive chemist who doubts the value of introducing them into more advanced stages of work. To do so would be to teach and learn a science of chemistry, with the science all left out.

A fair way to judge of the value of any discovery is to imagine that it had not been made, and see how the science would be affected by its absence. Similarly, in dealing with a question like the one under discussion, it would seem to me that a logical way to approach it would be to ask, What is lost by not incorporating the modern advances into elementary chemistry, and then what is gained by doing so?

It is certainly true that if we omit these generalizations from the early stages of chemical work we are teaching something that is out of date. There can be no two opinions on this point. But this alone does not solve our problem.

Perhaps the science as developed twenty-five years ago is better adapted to teaching the beginner than is the chemistry of to-day. It is certainly simpler. Why not teach the first year student in chemistry, in addition to a judicious number of the empirical facts of the science, something about the atom and the molecule, and leave it for a later stage to present the more recent developments? What would be lost by so doing? We have now arrived at a fundamental question.

The answer to this question is, in my opinion, that we must no longer teach the chemistry of three or four decades ago, because we know that in many fundamental points *it is untrue*. But it might be answered, we grant you this, but for the sake of *simplicity* we will teach the old chemistry, for, say a year, and then turn the student over to the new.

It is right here that an insuperable difficulty is encountered. It is the *persistence of first impressions*. Any one who has observed this at all carefully knows how nearly impossible it is to correct erroneous first impressions. Whatever the physiological or psychological explanation of the persistence of these impressions may be, the fact remains.

I have had this brought home to me so often and in such a forcible manner that it has made a deep and lasting impression. It has been my lot to try to teach something of the newer developments in chemistry to some students who have been trained in the older school. The result has been that it has required years of incessant drilling to ingraft the new generalizations into the mind of such a student. At first, the newer conceptions were scarcely more than tongue deep. In answer to questions it would be stated at first that "it is said" that such and such is true, or "the book says," or "you said" that this or that is the explanation; all of which went to show that the new ideas had penetrated hardly more than skin deep, and this, notwithstanding a serious effort on the part of an honest student to make the real science of chemistry an integral part of himself.

What is the explanation of this rather distressing condition of things? *Erroneous first impressions*, from which it is almost impossible wholly to escape.

There is one other matter to which I should like to refer before leaving this part of the discussion. This is the tendency which has existed in the past in this country to make chemistry *easy*. I do not believe there can be much difference of opinion as to this being a fact. How often and how justly have we heard the elementary course in chemistry branded by the student body as a "snap"; and for this very reason a preponderating number of students elect this course.

This condition is nothing less than *fatal*, as far as the science of chemistry is concerned; and every serious teacher must study its cause and apply the remedy.

How has this condition come about? Largely, I believe, as follows: A quarter of a century ago chemistry was almost wholly an empirical branch of science. Rowland used to say that chemistry in his day was in the same stage of development as physics in the days of Michael Faraday; and this was only a slightly exaggerated statement. It was necessary at that time to present the subject of chemistry largely by the empirical method. The result was with chemistry, as with any other empirical branch of science, the comprehension of the subject involved primarily, and may I say chiefly, the memory. A reasonably developed memory is much more general than equally well developed reasoning powers, and the use of the latter involves the expenditure of far more mental energy than the use of the former. This is the reason why chemistry was regarded as *easy*. It was something that could be readily memorized.

While this was perhaps a more or less necessary condition, several decades ago, those conditions are now largely changed. Chemistry is rapidly advancing along the way to become a branch of exact science, and it can be dealt with to-day in no small measure by the deductive method.

Far be it from my purpose to make chemistry *hard*, or even harder than is necessary for the best good of the science, at least in the early stages of the study of the subject; but a

far more important object than to make chemistry easy is to make it scientific. The object of the teacher should be to make the subject *clear*, but I have not very much respect for making things easy, since in science whatever is *easy* is *superficial*. There is no inherent reason why we should make elementary chemistry appreciably easier for the average student than elementary physics; that is to say, make it more superficial.

The argument against introducing the newer generalizations into the elementary teaching of chemistry, based upon the fact that their omission renders the subject easier, is, then, in reality a strong argument in favor of incorporating them.

The question as to whether it is easier for the teacher to introduce or omit the newer conceptions does not enter into the present discussion, since every efficient teacher is abreast with the development of his science; and furthermore, in matters of teaching, it is only the best good of the student that is to be considered.

Let us now turn to the other question: What is *gained* by teaching elementary chemistry from the standpoint of the newer generalizations?

A beginner in chemistry soon learns that when a chloride is treated with concentrated sulphuric acid, hydrochloric acid gas escapes, and the chloride is transformed into the corresponding sulphate. At one time this was explained as due to the greater *strength* of sulphuric acid; but we can not offer this explanation any longer, since we now know that sulphuric acid is only a little more than half as strong as hydrochloric.

The same beginner quickly learns that when a solution of a chloride is treated with a solution of silver nitrate, insoluble silver chloride is precipitated.

These two classes of phenomena are typical of a large number of chemical reactions. In the past such facts were summarized by saying that whenever a gas can be formed it is formed, and whenever a solid can be formed it is formed. This was simply *renaming* the phenomena in question, but of course explained nothing. Yet it was the best that could be done at that time.

It is a very simple matter to give any one, and therefore a beginner in chemistry, some qualitative conception of the effect of mass or quantity on chemical reactions—chemical reactions being dependent upon two things, the nature of the substances brought together, and their relative quantities. If the beginner can grasp one of these conceptions he can grasp the other.

Given the conception of mass and even qualitatively its function in chemistry, the two typical reactions mentioned above can be interpreted or, indeed, explained.

Hydrocloric acid having a low boiling point is a gas at ordinary temperatures, and escapes from the field of action almost as rapidly as it is formed; its active mass being thus reduced nearly to zero.

The silver chloride formed is nearly insoluble in water. It is precipitated as a solid and its active mass is thus small. I think this treatment renders the two typical reactions more clearly understood, and is more scientific than simply renaming the phenomena.

I venture to predict that not a few students of chemistry, not only of one year's standing but of several, are without any adequate conception of the importance of that condition in which matter in a given state of aggregation is, when mixed with matter in the same or a different state of aggregation—in a word, of the importance of *solution*.

If they were told that the whole science of chemistry is a branch of the science of solutions, they would either not understand the statement at all, or would regard it as a gross exaggeration.

It is a simple matter to make this reasonably clear, at least towards the end of the first year's work in chemistry. By that time enough reactions have been studied to show the student that practically all, if not all chemical reactions take place in solution, using the term solution in the broad sense in which it is employed to-day. Matter in the pure homogeneous condition is scarcely

capable of doing anything chemically, and that the science of solutions is much broader than chemistry will be seen after a moment's reflection. Geology is largely a science of solutions—of aqueous solutions and molten magmas, and how many branches of the biological sciences owe their existence to matter dissolved in other forms of matter?

In the pure homogeneous condition matter is, as we have stated, relatively inert. Nature, and, consequently, the science of nature, is, as it is, primarily due to matter in the dissolved state; and our knowledge of solutions, thanks to Van't Hoff and Arrhenius, is now reasonably satisfactory. We know far more about matter in the gaseous state than in the liquid or solid state. Van't Hoff has shown us that we can deal with solutions in many fundamental respects as we deal with gases. Consequently, we know far more about matter in solution than in the pure homogeneous liquid or solid condition. Why should these facts be concealed from the student of chemistry until late in life?

And now we come to another fundamental matter—the nature of the units that take part in chemical reaction. For a long time it was taught that the atoms and the molecules are the active chemical agents, and this was in keeping with what was known at the time.

This is now largely changed. The number of concordant lines of evidence which show that electrically charged parts are necessary for chemical activity, is so great, that I know of no productive chemist to-day who seriously questions it. After thinking over this problem and working upon it for a good many years, I am of the opinion that there is no chemical reaction known to man in which at least one of the substances taking part in the reaction is not more or less ionized. Indeed, I am unable to form any physical conception of even the posibility of a chemical reaction between electrically neutral parts, any more than I can form a conception of two electrically neutral bodies attracting or repelling one another electrically. It would lead us much too far to discuss at all fully this question here, nor is it necessary to do so. To furnish evidence to-day for the general truth of the theory of electrolytic dissociation, would be as unwise and as useless as to furnish new evidence for the law of the conservation of energy, or for the law of the conservation of mass.

In the light of these facts are we justified in continuing to teach the beginner the old chemistry of atoms and molecules, which we know, or should know, is untrue; trusting to later years, to new experiences, or to another instructor to correct these erroneous first impressions, which, as has been stated, is well nigh impossible.

Take another phase of things. A phenomenon which must be encountered very early in the study of chemistry is *precipitation*, already referred to in another connection. Has it been possible to treat this subject scientifically until quite recently? I think not. Whenever a precipitate could be formed it was formed, was about the way this matter was left. In the chemical reaction in question a solid is formed, which is practically insoluble in the solvent used; and being insoluble it is thrown down in that coarse-grained condition that we call a precipitate.

Think of this for a moment. When the solid was formed it was probably in a state of molecular aggregation. How do these solid molecules know enough to come together and form aggregates of the sizes that exist in precipitates? Furthermore, if this is the "natural condition" of insoluble solids when formed in a chemical reaction, then why do we not *always* have precipitation when an insoluble solid is formed in a reaction? In a word, why do we have in some cases *colloidal suspensions?*

To fix the idea and by way of illustration, why is arsenic sulphide precipitated when arsenic chloride is treated with hydrogen sulphide, but is not precipitated when arsenic oxide of the same concentration as the chloride is treated with hydrogen sulphide? Not only must every teacher of chemistry have asked himself this question, but every intelligent student, before he has advanced very far, must do so.

This is now very satisfactorily explained by another really great man of science—a man whose work for chemistry is quite as fundamental as his work for physics—I refer, of course, to Sir J. J. Thomson.

He has shown that whether or not precipitates are formed is dependent upon the presence or absence of appreciable numbers of charged parts or ions. Arsenic sulphide is precipitated from the solution of the chloride because the hydrocloric acid set free by the action of the hydrogen sulphide is strongly ionized. On the other hand, arsenic sulphide is *not* precipitated from the solution of the oxide, because no strongly dissociated substance is formed as the result of the reaction, and neither arsenic oxide nor hydrogen sulphide is strongly dissociated.

But Thomson does not stop with showing that ions or charged parts are necessary for precipitation. It was shown by Burton, working in Thomson's laboratory, why, or at least how, this is the case. Space will not allow me to go into this in detail. Suffice it to say here that the colloidally suspended particles are charged electrically, and for any given colloid all of the particles are charged with the same sign. These electrical repulsions work counter to surface-tension, which acts so as to draw the particles into the smallest surface for a given mass—to draw the colloidally suspended particles into lumps as in an ordinary precipitate. When ions are present these electrically neutralize the charges upon the colloidal particles and allow surface-tension to produce its full effect.

That ions are necessary and sufficient to effect precipitation, can readily be shown by adding almost any electrolyte to the colloidally suspended particles of arsenic sulphide, obtained by treating the oxide with hydrogen sulphide. A precipitate is formed at once.

This work places the whole subject of precipitation, for the first time, upon a scientific basis, and while it can not be presented fully to a beginner, I see no reason why it should not be judiciously taught to a student in his second year of chemistry, *i. e.,* when he is studying qualitative and quantitative analysis.

Then arise some of the most fundamental problems. What is a chemical atom? If made up of parts what are these parts, and how are they arranged within the atom? How does one chemical atom differ from another chemical atom? Are the chemical atoms stable?

These matters must all be taught the student of chemistry and the question is when? They can not of course all be presented fully to what we ordinarily mean by a beginner, but I can see no reason why they can not be presented, in an elementary manner of course, at the proper places, even in the first year's work in chemistry, unless we are wedded to the dogma that chemistry must be made *easy* in order that it may be *popular.*

We can certainly no longer teach that the chemical atom is an "ultimate unit" in the light of the recent work of Thomson. We know that it is made up of parts, and furthermore, we have some idea how these parts are arranged in two dimensions in space in a section through the atom. We have very good reason to believe that most, if not all of the differences between the atoms of the various chemical elements are a function of the number, arrangement, and possibly the velocities of the electrons composing the atoms. And why not, in a common-sense manner, tell the student of chemistry so, even in the comparatively early stages of his work?

Indeed, I think it is far simpler to teach this fundamental connection between the elements, than to have the beginner look upon the eighty or more elementary substances as so many discrete, disconnected, and fundamentally unrelated kinds of matter—to say nothing of it being true; and in the teaching of science I think *truth* is even more important than *simplicity.*

And again, take the question of the *stability* of the chemical atom. The stable atom of the past is now hardly more than historically interesting. The work of the Curies and especially of Rutherford, on radioactive sub-

stances, has placed this almost beyond the pale of doubt. The atoms with the largest atomic masses are certainly unstable, and it is highly probable that the atoms of all the elements are undergoing revolutionary changes.

In the light of these facts are we going to persist in teaching the stable atom, without qualification even to the beginner, and rely upon time, fate or the effort of some one else to correct, if possible, the evil that we have done ? It is perfectly true that the stable atom is simpler for the beginner than the unstable atom, but here again it is *simplicity vs. truth.*

In conclusion, there is one other matter which I can not leave untouched, because it lies at the very foundation of our science. I submit that no serious student of chemistry, and this is the class for which we must be most concerned, can study the subject for six months, learning that certain things react chemically with certain other things, and that certain things do not react with one another, without asking himself the question, why is this? Why do some substances react, and why do others not react? If this question is not raised by the student it certainly should be by the instructor. The question then is, why do chemical reactions take place at all?

We might almost call this the most fundamental question of chemical science. It is certainly so for the student, and that in the early part of his career. This brings me to the most heterodox position that I have yet ventured to take.

Should we not introduce into our elementary courses in chemistry something about the *energy changes* that take place in all chemical reactions, and which make those reactions possible? In the evolution of chemistry the material changes were studied first, and this was natural. These changes were the most obvious, and the material products were often desired for one purpose or another. Again, these material changes were the easiest to study, and chemists, like other men, were inclined to follow the lines of least resistance. I believe the nineteenth century will go down in the history of chemistry primarily as the period of *material chemistry.*

But even this is changed now. Without decrying in the least the study of matter, the chemist of to-day insists that we can no longer ignore the changes in energy that manifest themselves in every chemical reaction. Indeed, he would even go further, and point out again that whatever is easiest in science is relatively most superficial.

We know to-day that *all* chemical reactions are really due to differences in the intensity, or quantity, or kind of the intrinsic energy present in the substances that are to react; and whether any two substances will or will not react is determined primarily by this difference. We can, furthermore, form a physical conception now of what is meant by intrinsic energy, since we have the electron theory of the atom; it is primarily the kinetic energy of the moving electrons within the atom.

But dare we venture even to refer to energy or energy changes in the early stages of the teaching of chemistry? I ask why not? The physicist does not hesitate to do so. Indeed, most of his subject has to deal very largely with changes in the different manifestations of energy. Why should we assume that the chemical student has less natural intelligence than the student of physics, especially when he is almost always the same student? (In my opinion no one should be allowed to begin the study of chemistry until he has had at least one year of physics.

There is, of course, no reason for assuming that the beginning chemist is not as intelligent as the beginning physicist, and, therefore, there is no more reason why a student of chemistry should not deal with changes in energy than a student in physics, especially when these energy changes are as fundamental for chemical science as they are for physics.

Instead of teaching to-day that chemical reactions are *accompanied* by energy changes, why not teach the truth, which is, that it is these very energy changes that are the cause of all chemical reaction? Systems which alone are fairly stable, when brought together

may become unstable. There is a running down of a part of the intrinsic energy of one or both of the substances into heat, light or electricity but almost always largely into heat; and the substances rearrange themselves into those new combinations which are most stable under the new conditions.

This is what we ordinarily describe as a chemical reaction, and this can be taught to any sensible student just as well as the elements of physics can be taught to him.

Finally, the matters herein referred to, together with many others which time will not permit me even to mention, can not, of course, be taught the beginner all at once, in addition to the so-called material facts of chemistry. It is, however, a fair question to ask whether some of these matters would not be a fair substitute for a part of the pyrotechnics that sometimes adorns the chemical lecture table?

In all such matters the judgment and common sense of the teacher must of course be the final guide, and the intellectual fiber of the student must also be taken into account. It goes without saying that we must not teach dogmatically anything to the student of chemistry, much less to the beginner in chemistry, that is not reasonably substantiated; but I believe that all of the matters referred to above and many more of their type belong in this class.

The final question then is, shall we have two chemistries or one? Shall we have a chemistry of research, pushing forward at a pace that makes the last twenty-five years mark a distinctly new epoch in the history of the science? and another chemistry taught the beginner, which practically ignores all that has been done within that period; which deals not only with what is obsolete, but with what we know to be largely untrue, and which relies upon subsequent teaching to do almost the impossible, *i. e.*, correct erroneous first impressions, which must in some method be corrected, or the result is fatal?

Or shall we have one science of chemistry? Research leading the way, and teaching following fairly closely behind? At least doing nothing that will have to be undone, but incorporating what is truest and best.

For those who believe as I do that the latter is the more scientific course, there is not only no ground for pessimism, but not even for pragmatic meliorism.

The progress in this direction during the last decade, not only in the better colleges and universities, but in the more progressive high schools, has been so rapid that there is room for nothing but the most cheerful optimism.

Harry C. Jones

IS SCIENCE REALLY UNPOPULAR IN HIGH SCHOOLS?

The period covered by the tenth decade of the nineteenth century and the first of the twentieth was one of great activity in the reconstruction of high school schedules. The reports of the N. E. A., Committees of Ten and on college entrance examinations, the formation of the College Entrance Examination Board, the Perry and other movements for the reform and unification of science and mathematical teaching, all must have influenced high school curricula, and the alterations of the curricula must have shown effects in the percentages of secondary students in the various courses.

The famous attack made by President G. Stanley Hall[1] on the methods and attitude of secondary teaching in the United States was based to a certain extent on the summary tables of the percentage of secondary students in the United States taking the various high school studies, and published in the reports of the Commissioner of Education, 1890 to 1907. In order to exhibit these I have plotted the data on a chart. The curves for studies, graduates and college preparatory students are from the summary table (p. 1052), Report of the Commissioner of Education for 1907; that for per cent. of secondary students

[1] G. Stanley Hall, "How Far is the Present High School and Early College Training adapted to the Needs and Nature of Adolescents?" N. E. Asso. Coll. and Prep. Schs., 16, p. 72, 1901; *Ped. Sem.*, 9, p. 92, 1902; *Sch. Rev.*, 9, p. 649, 1901.

in the total population, from table on p. 1044. No curves are plotted for trigonometry and psychology, as they have never been of appreciable importance as high-school studies; the curves for Greek and geology are omitted, as they so nearly coincide with that for astronomy as to cause confusion.

Certain facts stand out from the curve-sheet. Greek has declined; so has civics; Latin, modern languages, English literature, rhetoric and foreign history have all increased, some of them enormously; *all the natural sciences* have fallen—geology, astronomy, chemistry, physics, physical geography and physiology have all dropped down, some of them enormously. Meanwhile the percentage of graduates has increased—a good showing, indicating that students are better satisfied with the schools than they were formerly—the proportion of students preparing for college has diminished, and the proportion of secondary students to total population has nearly doubled.

It is well known that the proportion of secondary students in the earlier years of the course is greater than that later. Hence a possible cause of an observed diminution in popularity of a subject is the alteration of schedules so as to shift the study into the later years of the course, and *vice versa* for an increase of popularity. Then the remarkable growth of the elective system, which occurred largely in the period covered by these curves, and the actual withdrawal of courses, are other causes which would affect the percentages. Now if we can in any way numerically express the opportunity which the average student has to take a given study, and compare with this the amount to which he takes advantage of his opportunity, as expressed in the tables of the bureau, we have in the ratio a numerical measure of the popularity of the study. I hope to be able to do this in a rough way from data already published, and to show that the drift away from science is in part at least the result of schedule tinkering, and does not completely express the taste of that much-criticized phenomenon, the rising generation.

If we can find the probability that a student selected at random from the mass shall be in any particular year of the high school course, and also the probability that a particular subject shall be offered by his school in that year, then the probability that this randomly selected student shall be taking this subject in this year is the product of these two probabilities, on the supposition that the subject is *required of all students* in this year. And this probability is also the percentage of students in the great mass who would be taking this subject in this year under the same supposition of no election.

The first probability is given by the Commissioner of Education in the Report for 1907, p. 1046, where it is said:

> For several years this bureau has estimated the proportion of secondary students in each of the four years as 43 per cent. in the first year, 26 per cent. in the second year, 18 per cent. in the third year and 13 per cent. in the fourth year. This estimate was based upon the enrollment of secondary students by grades in the high schools of a number of cities.

Two things show that this is not a constant distribution. First, the bureau has for three or four recent years collected data of this sort for the whole country, beginning with this report of 1907, and the figures do vary a fraction of a per cent. from these estimates. Second, the percentages for high-school graduates charted on the curve-sheet, show that the proportion of graduates in the high-school population has gradually increased, being 10.05 per cent. in 1889–90 and 11.87 per cent. in 1905–6. But in spite of this evident, though not very large, variation, we have no other means of getting at the facts, and will use these mean values as representing the probability of a randomly selected student's being in any particular year of the course.

The means for estimating the amount and effect of schedule tinkering is very incomplete. An article by Professor E. G. Dexter[2]

[2] E. G. Dexter, *Sch. Rev.*, 14, p. 254, 1906; "Ten Years' Influence of the Report of the Committee of Ten."

gives the only statistics available for this purpose, so far as I know. He collected the printed programs of schools for the period just preceding 1894, when the report of the Committee of Ten would not yet be effective, and for that ten years later, and compared the two. In his own words:

> For the earlier portion of the study 80 schools were covered: 35 in the eastern section of the country, 25 in the middle west and 10 each in the south and far west. For the period a decade later the number of schools was 160: 49 being in the east, 46 in the middle west, 30 in the south and 35 in the far west.
>
> Neither these numbers nor the particular schools studied were the result of arbitrary choice, but in most cases of dire necessity. Every available course of study for the years 1892 to 1894 was considered, and this was essentially true for the period ten years later. So far as possible, the same schools were considered at both periods; but, as indicated by the figures, many more schools were included in the later than in the earlier study. This was that errors due to accidental conditions might be reduced to a minimum. I have not thought it necessary in this paper to give the names of the particular schools studied, but will say that the list includes the high schools of nearly all the larger cities of the country; and that none of the smallest schools are covered is suggested by the fact that only those issuing printed courses of study are included. The part of the study covered by this paper has to do only with those recommendations of the special subcommittees (of the Committee of Ten) which bear upon the high-school curriculum.

The second factor, the probability that the student will have the opportunity to take a study in the year in which he happens to be, can in most cases be computed from Dexter's data, in an approximate sort of way. I will give the computation for German in some detail, as in it appear all the irregularities which show themselves in connection with other subjects, and further, the resulting table contains the only essential absurdity which developed in the preliminary computations.

First, Dexter's table for German.

TABLE I

	1894	1904
Percentage of schools offering 2 years	34	25
Percentage of schools offering 3 years	33	36
Percentage of schools offering 4 years	33	23
Percentage beginning in the first high-school year or earlier	48	47
Percentage beginning in the second high-school year	30	41
Percentage beginning in the third high-school year	22	12

(I infer that 16 per cent. gave a 1-year course in 1904, beginning in year III.)

This table I rearrange and extend as follows:

TABLE II

1894					1904				
Begin	Per Cent.		Length	Per Cent.	Begin	Per Cent.		Length	Per Cent.
I.	48	33	4	33	I.	47	23	4	23
		15					24		
			3	33				3	36
II.	30	18			II.	41	12		
		12					29		
			2	34				2	25
III.	22	22			III.	12	−4		
		0					16		
IV.	0		1	0	IV.	0		1	16

The percentages opposite the half-braces ({) mean thus: For 1904 23 per cent. of the schools gave a 4-year course, while 47 per cent. began in year I.; hence 24 per cent. must begin a 3-year course in year I. Thirty-six per cent. gave a 3-year course, hence 12 per cent. begin the 3-year course in year II., etc. That this course of reasoning is imperfect appears from the fact that according to it —4 per cent. begin a 2-year course in year III., which is absurd. However, the difficulty lies in the original data being out of reach, and as the absurdity is not going to be of great influence on the computations, as it comes in the third and fourth years, I use the figures as they stand. The table for German is the only one in which any patent absurdity shows itself.

From this table I obtain the percentages which express the random student's opportunity to take German, *i. e.*, the per cent. of students who would be taking German were it required wherever and whenever it is offered, thus:

The probability that a student be in the first year of the course is 0.43, from the data of the Bureau of Education; the probability that German will be offered in that year is (1904) 0.23 for a 4-year course and 0.24 for a 3-year course, or 0.47 for both; the desired probability is then $0.43 \times 0.47 = 0.202$.[3] The probability that a student be in the second year is 0.26; the probability that German will be offered in that year is 0.29 for a 2-year course begun that year, 0.24 for a 3-year course begun in first year, 0.23 for a 4-year course, 0.12 for a 3-year course begun in second year. The resulting probability that a student be taking German, if it were a required study in second year, is $0.26\ (0.29 + 0.24 + 0.23 + 0.12) = 0.232$. Similarly for the other years. Then the total probability that a random student be taking German in 1904 is 0.654. Computations like this carried out for the studies in Dexter's tables result in the following table.

With regard to some of the other subjects tabulated by the Bureau of Education, I am not able to draw any conclusions from Dexter's tables and other data. Some of his facts may be quoted as supplementing the table above.

This goes far to explain the increase in the percentage of students studying foreign history, as tabulated in the commissioned report and shown on the curve-sheet.

Table III., in spite of the very inadequate data on which it is in part based, is capable of giving us a certain amount of information about the relations between election and schedule alteration and the data of the Bureau of Education. The columns headed "per cent.—if required" give in per cents. the probabilities for each study that a random student would be taking the subject if there were no elective system, as derived from Dexter's data, and hence also the percentages of students in the mass who would take the subject under these conditions. In a way they measure the average opportunity for a student to take the subject. The column "ratio" gives the quotient of the per cent. for 1904 by that for 1894. It measures the extent of schedule change. The columns headed "per cent.—actual" are quoted from the commissioner's table. The "ratio" column is found in the same way. The columns headed "actual/required" check the accuracy

TABLE III[4]

Study	Per Cent.—if Required			Per Cent.—Actual			Actual/Required	
	1894	1904	Ratio	1894	1904	Ratio	1894	1904
Latin	69.2	91.4	1.32	43.59	49.96	1.23	0.63	0.55
French	64.8	66.0	1.02	10.31	11.15	1.08	0.16	0.17
German	72.0	65.4	0.91	12.78	18.98	1.49	0.18	0.29
Algebra	47.7	53.1	1.11	52.71	56.23	1.07	1.10	1.06
Geometry, plane	24.2	30.4	1.26					
" solid	8.0	10.5	1.31					
" both	32.2	40.9	1.27	25.25	27.30	1.08	0.78	0.67
Physics	21.3	22.0	1.03	24.02	15.90	0.66	1.13	0.72
Chemistry	11.5	10.2	0.89	10.31	7.08	0.69	0.90	0.69
Geology	11.0	5.6	0.51	5.52[5]	2.79	0.51	0.50	0.50
Physical geography and physiography	29.8	27.0	0.91	22.44[5]	21.26	0.95	0.75	0.79

[3] In this it is assumed that all schools dealt with are of the same size, which is inaccurate, but unavoidable.

[4] Results computed from the articles of Hunter, Weckel, Ramsay and Whitney, published in *School Science and Mathematics* during the last two years, supplement the above table in part. But they depend on limited or fragmentary data. A complete census by the Bureau of Education would be of great value.

[5] 1894–5 data.

of the methods used, and show that in three places there are errors; for it is manifestly impossible that in 1894 or 1904 there could be more students taking algebra or physics than would take it were the subjects required. Besides, these columns tend in the clearest way to show the effect of the elective system; the ratios measure in a rough way the popularity of a study, when it is elective.

TABLE IV

Study	Per Cent. Schools Offering 1894	1904
Latin, four years	46	80
Physics	97	100
Chemistry, mostly in year III.	74	66
Astronomy	63	31
Physiology, mostly in years I. or II.	81	57
Trigonometry	23	44
English,[6] four years	52	68
English, less than four years, more than three years	12	32

For example: in spite of the great increase in the percentage of students taking Latin, the subject had in fact declined in popularity, as shown by the ratios, 1894, 0.63, 1904, 0.55; further, these ratios show that, after all, relatively few students took the subject in comparison with the opportunities.

TABLE V

The History Branches

	1894		1904	
	Per Cent. Offering	Length of Year	Per Cent. Offering	Length of Year
American	57	0.7	86	0.64
French	0	0.0	7	0.50
English	39	0.5	51	0.66
"Intensive"	0	0.0	5	1.00
Greek	47	0.5	57	0.50
Roman	50	0.5	57	0.50
General	46	1.0	61	1.00

Examining the table with this in view, we see that French has hardly changed, while German draws increasingly on the affections of secondary students—may this be a reflex of the great influence of German thought in the universities, brought into the secondary schools by college-bred teachers? But both French and German have a low popularity, lower than any science. Algebra is the universal study, not generally elective, and so it is not surprising that its "popularity" should be represented by a number in the neighborhood of 1. The excess gives a rough idea of the errors inherent in my data, and the amount of guessing which has crept in. Geometry was in 1904 not so generally required as in 1894, and so shows a fall of popularity. Physics was in 1894 more generally required than in 1904, which accounts in part for the drop from about 1.00 to 0.72. The balance means real dislike for the subject. Geology and physical geography and physiography stay about where they were in popularity.

I am inclined to conclude from this table that, in spite of a general impression to the contrary, American boys and girls like the sciences, both exact and natural, better than they like the languages, *provided they only have as good a chance to get at them;* and the way to save the situation for science is to give them the chance early in the course. I assert with confidence that, had 80 per cent. of Dexter's schools in 1904 offered four years of chemistry and physics, instead of four years of Latin, as they did, we should have found the figures of percentages just about reversed, or even worse for Latin.

WILLARD J. FISHER

ITHACA, N. Y.,
December, 1911

[6] The separation into English literature and rhetoric or composition is not sharp, and is hard to tabulate.

THE SMITHSONIAN BIOLOGICAL SURVEY OF THE PANAMA CANAL ZONE

THE Biological Survey of the Panama Canal Zone, begun in December, 1910, and continued through the major part of 1911, is being pushed to completion before the opening of the canal in 1913. The second expedition sailed on January 9, to take up the work for another season, the botanist, Professor

Pittier, being the only naturalist who has remained in the field since the beginning of the survey. Although much interesting information has been collected, and a great many specimens secured, nothing like a complete report is ready.

The party will include Dr. Seth E. Meek, formerly of the Bureau of Fisheries, but now representing the Field Museum of Natural History; Mr. S. F. Hildebrand, of the Bureau of Fisheries, who will collect fishes, reptiles and amphibians; E. A. Goldman, of the Biological Survey, Department of Agriculture, who will collect birds and mammals, and Professor Charles D. Marsh, of the Bureau of Plant Industry, Department of Agriculture, who will collect and study the microscopic plant and animal life of the fresh waters of the zone.

Leaving New York on the steamship *Panama*, they will proceed to Cristobal, Canal Zone, their headquarters on the Atlantic coast, and there make preparations for a sojourn of four or five months in the field.

The life-areas on the zone will become confused as soon as the canal is opened and the waters of the Pacific and Atlantic watersheds are intermingled. It is particularly important on that account, that the present geographical distribution of animals and plants be recorded prior to that time, and this is especially true as regards the life of the fresh waters and the sea-coasts.

The work of the survey is carried on through the united efforts of the Smithsonian Institution, several of the government departments and the Field Museum of Natural History of Chicago, and the hearty cooperation of the Panama Canal Commission has been an important factor in the success of the undertaking.

As a preliminary of the work already accomplished, the Smithsonian has published four pamphlets. The first two (Nos. 2015 and 2053 of the Smithsonian Misc. Colls.) are by E. W. Nelson, of the Biological Survey, and describe a new humming-bird, a motmot and a bird of the genus *Pachysylvia*. The third, by E. A. Goldman, one of the naturalists of the survey, contains a description of a new kingfisher.

Mr. Maxon, of the Division of Plants, National Museum, who accompanied Professor Henry Pittier to the Canal Zone last year, has published a description of a remarkable new fern (Smiths. Misc. Coll. No. 2055).

After all the new forms of animals and plants have been described it is proposed to publish general accounts of all the various collections and also one or more volumes containing a summary of the whole fauna and flora of the Canal Zone.

SCIENTIFIC NOTES AND NEWS

DR. J. A. ALLEN, curator of mammalogy and ornithology in the American Museum of Natural History, has resigned the editorship of *The Auk*, and the council of the American Ornithologists' Union, at the recent meeting in Philadelphia, chose Mr. Witmer Stone as his successor. Simultaneous with Dr. Allen's retirement Mr. Frank M. Chapman resigned as associate editor. Beginning in 1876 with the initial volume of the *Bulletin* of the Nuttall Ornithological Club, Dr. Allen guided the course of this journal and its successor *The Auk* since its establishment in 1884.

DR. J. WALTER FEWKES, of the Bureau of American Ethnology, has been reelected president of the American Anthropological Association. The next annual meeting of the association will be in Cleveland, Ohio, beginning on December 30, 1912, in affiliation with Section H of the American Association for the Advancement of Science.

DR. THEOBALD SMITH, professor of comparative pathology at Harvard and exchange professor at the University of Berlin during the present academic year, delivered his first lecture on January 8. His subject was "The Relation between Parasitism and Disease."

PROFESSOR GEORGE GRANT MACCURDY will be the delegate from Yale University to the eighteenth International Congress of Americanists to be held in London from May 27 to June 1, 1912.

Mr. Alex. Wetmore, of the Biological Survey, U. S. Department of Agriculture, is in Porto Rico this winter, cooperating with the Insular Board of Agriculture in studying the economic relations of the birds and mammals of the island.

Professor A. S. Hitchcock, systematic agrostologist, U. S. Department of Agriculture, has returned from Panama. About two months were spent in the Canal Zone and other parts of Panama, where he was a member of the Smithsonian Biological Survey, and about two months in the five Central American Republics. Nearly 200 species of grasses, represented by 565 numbers, were obtained in Panama and 760 numbers of grasses in Central America. While in Panama a trip was made to the extinct volcano Chiriqui, which has an altitude of a little over 11,000 feet. Besides several novelties there were collected many species heretofore known only from South America. Mr. Hitchcock was accompanied by his son, Frank H. Hitchcock, with whose aid he was able to obtain many duplicates.

We learn from *The Auk* that Mr. Samuel N. Rhoads has returned from Ecuador after collecting at various points along the railroad which runs from Guayaquil to Quito, especially at Bucay (975 feet), on the Chanchan River (2,000 feet), Huigra (4,000 feet), Mt. Pichincha (8,000 feet), Riobamba (10,000 feet) and in the vicinity of Quito. He brought back about 1,600 birds, some mammals and reptiles and a number of invertebrates. His collection is now at the Academy of Natural Sciences, Philadelphia.

Professor L. Hektoen, of the University of Chicago, gave six lectures on the Herter foundation of the University and Bellevue Hospital Medical College, beginning on January 8. The subject of the lectures was "Immunity."

Dr. William H. Welch, professor of pathology at the Johns Hopkins University, will give the lectures on the Barbour-Page foundation at the University of Virginia.

Mr. Gano Dunn, president of the American Institute of Electrical Engineers, was the guest of honor at the annual dinner of the Ithaca Section of the institute, held on January 10. His subject was "The Kind of a Man that makes a Good Engineer."

Mr. C. A. Seley, mechanical engineer of the Rock Island Lines, delivered an address before the students and faculty of the College of Engineering of the University of Illinois, on January 4, 1912, on "Conference Committee Methods in handling Railway Legislation on Mechanical Matters."

Before the Geographic Society of Chicago on January 12 a lecture was given by Professor R. H. Whitbeck, of the University of Wisconsin, the title of the lecture being "Geographical Names and the Stories they Tell."

At the dedication of the Gauss monument on the Hohenhagen, near Dransfeld, the memorial address was delivered by Professor W. Voigt, of the University of Göttingen.

The daily papers announce that Dr. Simon, the bacteriologist of Zurich, has died as the result of a bite from an innoculated mouse.

The death is also announced of Dr. Stephan Lindeck, member of the Charlottenburg Reichsanstalt.

The Society of American Bacteriologists, at its recent meeting in Washington, elected the following officers:

President—Dr. William H. Park, director of the Research Laboratories, City of New York.

Vice-president—Professor C.-E. A. Winslow, College of the City of New York.

Secretary-treasurer—Dr. Charles E. Marshall, Michigan Agricultural College, East Lansing.

Council—Dr. W. J. MacNeal, New York Post-Graduate Schools, New York City; Dr. Otto Rahn, Michigan Agricultural College, East Lansing; Dr. H. D. Pease, 39 West 38th Street, New York City; Dr. John F. Anderson, director of Hygienic Laboratory, Washington.

Delegate to the A. A. A. S.—Professor D. H. Bergey, University of Pennsylvania.

Dr. G. Stanley Hall, president of Clark University, is giving a course of six lectures on "The Founders of Modern Psychology"

before the department of psychology of Columbia University. The lectures are on January 16, 17, 23, 24, 30 and 31, at 4.10 P.M. The subjects are as follows:

Edward D. Zeller, *the scholar in his field.* The historian of ancient philosophy, his personality, his learning, his career, characteristics as a teacher, his essays, historical method and position, and an estimate of his achievements.

Edward von Hartmann, *the philosopher of temperament.* Personal reminiscences of his traits, his early writings, outline of his chief positions in his major and his minor contributions, critique of his type of pessimism.

Hermann Lotze, *the harmonizer.* The man and professor, his early work as physiologist and physician, the chief position taken in his Microcosm and his Ethics, his system as an expression of his character and its historical significance. Why he never left a school.

Theodor Fechner, *the animist.* His life, works, personal reminiscences, his mystic papers, his unique type of idealism, his lapse to spiritualism, the psychophysic law and what it meant to him.

Hermann von Helmholtz, *the ideal man of science.* His boyhood, growth and early manhood, his methods of life and work, his essays and addresses, the fourth dimension of space, characterization of the chief discoveries in his optics and acoustics and their results.

Wilhelm Wundt, *a scientific philosopher.* His early career, his methods of work, his early, and a glimpse at his later, writings, some criticisms and appreciations.

Nature states that the jubilee annual meeting of the Yorkshire Naturalists' Union was held at Heckmondwike, on December 16, at the place where fifty years ago the union had its birth. There were more than three hundred members present, including delegates from thirty-eight affiliated societies of the Yorkshire Naturalists' Union. The presidential address of Mr. Alfred Harker, F.R.S., on "Petrology in Yorkshire," was delivered. Mr. T. Sheppard resigned his position as honorary secretary, and in view of his nine years' work in that position was elected an honorary life member of the union. Mr. W. Cash was similarly honored. Mr. J. W. Taylor, of Leeds, was elected president for 1912. The new secretaries are Dr. T. W. Woodhead and Mr. W. E. L. Wattam, Technical College, Huddersfield. The annual meeting for 1912 will be held at Hull on December 14.

As we learn from the New York *Evening Post* an old landmark which has figured extensively in the medical and surgical progress of the city for nearly a half century is to be sold by auction on February 1. The property is the old Mott Memorial at No. 64 Madison Avenue, opposite Madison Square Garden. The memorial was founded in 1866 by the widow of Dr. Alexander Mott, who in his day was one of the foremost surgeons of this country. Many surgical and obstetrical instruments, invented by Dr. Mott, as well as numerous morbid specimens he collected during his travels, were partly destroyed with the burning of the Medical College on Fourteenth Street. His widow succeeded in gathering mementos of his life and placed them in the Madison Avenue house, which was incorporated as the Mott Memorial in 1866, and conducted for many years by Professor Alexander B. Mott. It contained a library of more than 4,000 volumes, exclusively on medical and surgical topics, which were consulted freely by students and physicians. In the will of Mrs. Mott there was a proviso that in the event that the property should become burdensome the trustees were directed to sell it. The Memorial, after several years of uncertainty, during which the trustees hoped a way could be devised to perpetuate it, was closed in 1909, and the books, instruments and plates were transferred to the New York Academy of Medicine, of which Dr. Mott was president for a long period.

THE fourteenth International Congress of Anthropology and Prehistoric Archeology, will be held at Geneva, Switzerland, during the first week of September, 1912. The last session of this congress was held at Monaco in the spring of 1906.

THE *American Museum Journal* reports that Mr. Stefánsson, of the Museum's Arctic expedition, has made a discovery of an archeological nature at his last winter camp near

Pt. Stivens, Parry Peninsula. According to his report a great deal of pottery is found upon old village sites, some at a depth of several feet. This pottery is of similar type to that found among and lately manufactured by some of the Alaskan Eskimos. Pottery has so far not been reported from any of the central and eastern Eskimos. It was formerly assumed that the presence of pottery among the Alaskan Eskimos was to be explained as indicating fórms copied from Siberian or neighboring American tribes. The recent discoveries of Mr. Stefánsson indicate that the art of pottery among the Eskimos must have been of ancient origin and at one time very widely distributed. Furthermore Mr. Stefánsson reports that other objects he finds are similar in type to those described by Professor Boas, discovered by Captain George Comer in ancient village sites in Southampton Island, Hudson Bay. These were also similar to objects recently discovered in Greenland, leading to the conclusion that older types of Eskimo culture must have been much more uniform throughout the entire stretch of Arctic America than at present. Mr. Stefánsson's find of similar objects on the west side of Hudson Bay makes it more probable that there was formerly but a single type of Eskimo culture from Alaska to Greenland.

To demonstrate the process involved in changing raw materials into finished products, the course in commerce at the University of Wisconsin maintains a commercial museum for the use of the students in the course. Detailed exhibits of almost every product that has any commercial value are included. Among the most instructive are those of cotton, wool, silk, the grains and their products, rubber, steel and aluminum products and structural fibers. Different forms of money used in all parts of the world, and a collection of coins representing the circulating media of some of the less civilized peoples, are interesting features of this museum.

Subjoined are the names of the members of the commission on resuscitation from shock, selected by the American Medical Association at the request of the National Electric Light Association. This is the result of a series of conferences on the subject held during the past year by representatives of the leading engineering societies, officials of the government, etc. *Resuscitation Commission:* Dr. W. B. Cannon (*chairman*), department of physiology, Harvard Medical School. *Nominated by the American Medical Association:* Dr. Yandell Henderson, department of physiology, Yale University; Dr. Geo. W. Crile, 214 Osborn Building, Cleveland, Ohio; Dr. S. J. Meltzer, Rockefeller Institute. *Nominated by the National Electric Light Association:* Dr. Edward A. Spitzka, professor of anatomy, Jefferson Medical College; Mr. W. C. L. Eglin, Philadelphia Electric Company. *Nominated by the American Institute of Electrical Engineers:* Professor Elihu Thomson, ex-president of the American Institute of Electrical Engineers, Lynn, Mass.; Dr. Arthur E. Kennelly, Harvard University; Mr. W. D. Weaver (*secretary*), editor *Electrical World*, New York City. A conference was held on December 16 by the president and secretary of the commission and some preliminary work was mapped out. These steps will be followed up by an early meeting of the full commission, probably in New York in January, after which the plans adopted for the investigation will be vigorously pushed. It is felt that the much-needed revision of rules and practise in regard to this highly important subject will now be taken up under the best auspices and that authoritative conclusions will be reached. The officers of the association are highly encouraged in knowing that the question will receive the serious attention of these eminent medical men and that they regard it as worthy of their special study.

UNIVERSITY AND EDUCATIONAL NEWS

Dr. John Grier Hibben, Stuart professor of logic, has been elected president of Princeton University.

Dr. and Mrs. Charles Waldstein, of Cambridge, England, have given $5,000 to Columbia University to establish lectures on the foreign policy of the United States.

THE trustees of Northwestern University have announced a gift of $8,000 from the estate of Mrs. Ellen Sage which is administered by Mr. N. M. Jones, for the establishment of three scholarships: one in the College of Liberal Arts, one in the Medical School and one in the Law School, to be known as the Rufus H. Sage scholarships.

THE degree of bachelor of business administration will hereafter be conferred on graduates of Northwestern University School of Commerce who have had two years' regular college work and have spent two years in the School of Commerce.

BEGINNING with September, 1914, the Schools of Mines, Engineering and Chemistry of Columbia University, which comprise the faculty of applied science, will be substantially graduate schools, a baccalaureate degree being required for admission. But students will have the privilege of following a combined collegiate and professional course in engineering as they now have in law, medicine and teaching. The strictly technical or professional course of study will be three years in length instead of four as at present.

THE trustees of Teachers College, Columbia University, have created a School of Practical Arts, to comprise the present Schools of Household and Industrial Arts and the departments of fine arts, music and physical education. To this end there has been constituted a faculty of education, comprising the dean and the professors whose work is largely in education, who are to direct the School of Education, and the faculty of practical arts, including the professors of fine arts, music, household arts, industrial arts and hygiene and physical training. To this latter faculty is entrusted the development of the new School of Practical Arts, which is to offer a new type of university education—a four-year course, comprising both academic and vocational courses.

THE technique of printing and publishing is the subject of a new course to be given in connection with the work in journalism at the University of Wisconsin, beginning in February. The course will consist of practical talks and laboratory work on typographical composition, engraving processes, printing and similar topics. The study is intended primarily for students of engineering, agriculture, commerce, pharmacy, chemistry and other technical subjects, who desire to familiarize themselves with methods of printing and publishing in order to contribute to or do editorial work on scientific, technical and trade publications. A course in technical and trade journalism, to include lectures and practise in all the details of the work of the editor and the contributor on scientific, technical and trade publications, has also been arranged to be given next year.

HERBERT SHAW PHILBRICK, assistant professor of mechanical engineering at the University of Missouri, has been appointed professor of that subject in the College of Engineering of Northwestern University.

DR. H. E. BUCHANAN has been appointed professor of mathematics at the University of Tennessee.

PROFESSOR GEORG FABER, of the Technical School at Stuttgart, has been called to a chair of mathematics at Königsberg.

DISCUSSION AND CORRESPONDENCE

THE ADMINISTRATION OF THE WEEKS ACT

TO THE EDITOR OF SCIENCE: In consideration of Professor Very's letter in SCIENCE of January 5, I wish only to bring to the attention of the readers of SCIENCE section 6 of the Weeks Act, which has been interpreted to require an actual examination by the Geological Survey and a report based thereon which shall consist of a showing of facts rather than an expression of opinion.

Section 6. That the Secretary of Agriculture is hereby authorized and directed to examine, locate and recommend for purchase such lands as in his judgment may be necessary to the regulation of the flow of navigable streams, and to report to the National Forest Reservation Commission the results of such examinations: *Provided*, That before any lands are purchased by the National Forest Reservation Commission said lands

shall be examined by the Geological Survey and a report made to the Secretary of Agriculture, showing that the control of such lands will promote or protect the navigation of streams on whose watersheds they lie.

Those who are familiar with the eventful history leading up to the passage of the Weeks Act know that the principle invoked in section 6 was absolutely essential both to insure the constitutionality of the measure and to secure its passage. The administrative officer, however keenly he may appreciate the spirit which encouraged the movement for the preservation of the Appalachian forests, can not disregard the plain letter of the law on the statute book.

Geo. Otis Smith

U. S. Geological Survey

SUGGESTIONS FOR THE CLEVELAND MEETING

To the Editor of Science: Regarding the preparations for the meeting of the American Association for the Advancement of Science at Cleveland next year, I desire to suggest the advisability of concentrating the places of meeting so far as practicable, in order that the meeting rooms may be more conveniently found, and persons who wish to pass from one meeting place to another in order to hear a large number of papers read, may be able to do so.

Much of the benefit of these meetings depends on easy access afforded them. For this reason, the best arrangements in many years was that provided in the Central High School at St. Louis. There the basement, and the first- and second-floor classrooms were used for the different sections. Geographers could in a minute's time pass out of their meeting place to hear a paper in the session of the economist and statistican, or *vice versa*. Strangers coming in the building found the directory at the entrance, which told where each section was meeting and the room. There was no wandering about the campus, as at Chicago where some of the sections were located on the third floor of buildings; nor was there any fear of intrusion or collision with professors who had classes to hear, as at the Institute of Technology, Boston; nor was there any wandering about the streets to find where particular sections met, as in Baltimore.

A central building with wide hallways, the posting of a large directory at some outside point on a thoroughfare and the placarding of rooms, with the placard standing at right angles to the door when closed, with somebody at hand to make additional placards as needed—these suggestions seem to me worth while considering to help make our Cleveland meeting one of the best, if not the best on record.

John Franklin Crowell

CHROMOSOMES IN WHEAT AND RYE

In my paper entitled "A Theory of Mendelian Phenomena"[1] I referred to rye as having a small number of chromosomes—"six, I believe," while wheat has "40 or more," and called attention to a possible relation of these supposed facts to the great difference in variability of these two species. This reference to chromosome numbers was made on the basis of a statement made to me some years ago by a student who had made some studies of the subject. Mr. Orland E. White, of the Bussey Institution, calls my attention to the studies of Overton and of Koerniche, which indicate that wheat has sixteen chromosomes ($2X$ number).

W. J. Spillman

Washington, D. C.

HOW A FALLING CAT TURNS OVER

To the Editor of Science: In your last issue Professor W. S. Franklin mentions having given a valid explanation of how a cat is able to light on his feet when he is dropped back downwards. He does not state what this explanation was; but gives in full a different valid explanation offered by Professor J. F. Hayford. No statement is made as to which explanation agrees with the actual performance of the cat, so it may be of interest to call attention to a set of kinematograph pictures of a falling cat, published as Plate II. of H. Crabtree's "Spinning Tops and Gyroscopic Motion." These pictures corroborate

[1] American Breeders' Association, Report VI.

fully the following explanation given in the accompanying text:

"Let us regard the cat as made up of a fore part and a hind part, whose moments of inertia I_1, I_2 are equal when the legs are fully extended at right angles to the body. The photographs given in Plate II. show that it first contracts its fore legs (thereby making I_1 less than I_2) and then turns its fore part round. This latter action necessitates the hind part being turned in the opposite direction (since the total angular momentum about the axis is zero) but to a less extent, since I_2 is greater than I_1. The animal then contracts its hind legs, extends its forelegs, and gives its hind part a turn. This necessitates the fore part being turned in the reverse direction but, again, to a less extent, since I_1 is now greater than I_2. It will thus be seen that by continued action of this kind the cat can turn itself through any required angle, though at no time has it any angular momentum about its 'axis.'"

The explanation offered by Professor Hayford, although a possible one, accordingly does not agree with the actual performance of a cat, as observed by photography.

J. R. BENTON

UNIVERSITY OF FLORIDA,
December 18, 1911

SCIENTIFIC BOOKS

The Wilderness of the Upper Yukon: A Hunter's Explorations for Wild Sheep in Sub-Arctic Mountains. By CHARLES SHELDON. New York, Charles Scribner's Sons. 1911. 8vo. Pp. xxi + 354; 4 colored and 46 half-tone plates; 4 maps, one in colors.

The distribution and relationships of the mountain sheep of Canada and Alaska present one of the most interesting and puzzling problems in North American mammalogy. For the purpose of obtaining more definite information on this subject Mr. Sheldon, a hunter-naturalist of well-known qualifications for such a task, spent the seasons of 1904 and 1905 in the Northern Rockies, exploring the Ogilvie, the Selwyn and Plateau mountains and the Watson River country in 1904, and the Pelly, Rose and Glenlyon mountains in 1905. As a narrative of exploration in practically new fields, the book is an important contribution to our knowledge of the physical conditions and natural history of the region traversed, aside from its bearing upon the special quest for which these journeys were undertaken. Its excellent literary form, its abundant and admirable illustrations and the author's enthusiasm and sympathy with his surroundings, add a value and a charm to his pages unusual in books of hunting adventure. Maps are given of the districts traversed, excellent half-tones illustrate scenic features and there are four colored plates from drawings by Carl Rungius of sheep and other big game.

The sheep of northern Canada and Alaska are quite different from the well-known bighorn of the Rocky Mountains of southern Canada, the United States and northern Mexico. The first northern form to become scientifically known was the *Ovis dalli* described by E. W. Nelson in 1884 from specimens collected in the upper Yukon region of Alaska. This sheep is pure white at all seasons except for adventitious staining from soil or vegetation; it is smaller and has less massive horns than the various forms of the Rocky Mountain bighorn.

In 1897 a black form was described as *Ovis stonei* from specimens obtained in the Cheonee Mountains south of the Stikine River in northern British Columbia. Although the Alaska form is pure white, and the other so dark colored as to be known as the black sheep, the structural differences that characterize them are slight and inconstant.

A few years later (in 1901) a sheep intermediate in coloration between the white and black sheep was described as *Ovis fannini,* based on specimens collected near Dawson City. As the sheep of this general region became better known it was found that the sheep of the *fannini* type were very unstable in respect to coloration and were apparently intergrades between the white form of Alaska, the Yukon and Northwest territories and the black form of northern British Columbia.

This was about the sum of our knowledge of these sheep when Mr. Sheldon set out in 1904 and 1905 to make a special study of the sheep question of the northern Rockies, and to trace out their geographical and physical relationships. Chapter XX. of this book gives a summary of the results of his two seasons' work, and is illustrated by a map in colors showing the known distribution of the white and black sheep of Canada and Alaska, their areas of intergradation and the phases characteristic of special districts. Facing the map are half-tone figures of nine stages of color variation, with explanatory text. The subject is thus graphically and clearly illustrated by the distribution map, the facing explanatory text and shaded figures. The area embraced extends from about latitude 55° to latitude 70°. In Alaska, from the Arctic coast south to latitude 60°, and in Yukon Territory and northeastward in the Mackenzie Mountains to about latitude 62° (generally speaking), the sheep are pure white, except in the Tanana Hills south of the Yukon River, where the white coat is varied with a few black hairs and slight indications of the color pattern of the *fannini* type; in British Columbia south of the Stikine River the sheep are uniformly black; but over an intervening region of from approximately six hundred and fifty miles north and south and about one hundred and fifty to two hundred miles east and west, "there is no area in which the color of the sheep is uniform."

Mr. Sheldon indicates on his map five areas (*a, b, c, d, e*) where the sheep are either pure white (*a*), or black (*e*), or are of intermediate or mixed shades (*b, c, d*); the *b* grade is nearly white, the *d* grade nearly black, *c* being the middle phase or the *fannini* type, which is intermediate geographically as well as in color.

The facts of intergradation are thus forcibly and clearly presented—an intergradation continuous and gradual from one extreme phase to the other through a vast expanse of country. The cause of this extensive and gradual merging of these two widely diverse color types of sheep is not so easily demonstrable. Has it resulted from interbreeding or is it due to environment? Mr. Sheldon favors the former hypothesis, but admits the possibility of its having been "produced by subtle and indeterminate changes of environment to a much greater extent than the facts seem to me [him] to warrant."

The large size of these animals and the striking color differences between the extreme phases that are thus shown to intergrade render this an impressive instance of intergradation, but parallel cases, though less striking, in other animals usually seem explainable satisfactorily, and in many instances beyond question, on the hypothesis of the action of diverse conditions of environment. But whatever conclusion may finally be reached as to the cause, great credit is due Mr. Sheldon for his contribution of facts through a successful reconnoissance of the almost inaccessible haunts of the sheep in the Northern Rockies where lay the key to the problem—an undertaking few would have the hardihood to project or the endurance and persistence to accomplish. Besides the facts of variation and range already outlined, his contribution to the life-history of these animals is of noteworthy importance, while the wide range of individual variation among members of the same herd, not only as regards coloration, but in respect to size, shape and curvature of the horns is noted in detail. He has also presented to the National Museum the large series of specimens of sheep obtained by him on his expeditions which go far to substantiate the facts of intergradation recorded and illustrated in his book, which may be read with equal interest by the naturalist, the big game hunter and the general reader.

J. A. Allen

Principia Mathematica. By Alfred North Whitehead, Sc.D., F.R.S., Fellow and Lecturer of Trinity College, Cambridge, and Bertrand Russell, M.A., F.R.S., Lecturer and late Fellow of Trinity College, Cambridge. Cambridge University Press. 1910. Vol. I., pp. xiii + 666.

Mathematicians, many philosophers, logicians and physicists, and a large number of other people are aware of the fact that mathematical activity, like the activity in numerous other fields of study and research, has been in large part for a century distinctively and increasingly critical. Every one has heard of a critical movement in mathematics and of certain mathematicians distinguished for their insistence upon precision and logical cogency. Under the influence of the critical spirit of the time mathematicians, having inherited the traditional belief that the human mind can know some propositions to be true, convinced that mathematics may not contain any false propositions, and nevertheless finding that numerous so-called mathematical propositions were certainly not true, began to reexamine the existing body of what was called mathematics with a view to purging it of the false and of thus putting an end to what, rightly viewed, was a kind of scientific scandal. Their aim was truth, not the whole truth, but nothing but truth. And the aim was consistent with the traditional faith which they inherited. They believed that there were such things as self-evident propositions, known as axioms. They believed that the traditional logic, come down from Aristotle, was an absolutely perfect machinery for ascertaining what was involved in the axioms. At this stage, therefore, they believed that, in order that a given branch of mathematics should contain truth and nothing but truth, it was sufficient to find the appropriate axioms and then, by the engine of deductive logic, to explicate their meaning or content. To be sure, one might have trouble to "find" the axioms and in the matter of explication one might be an imperfect engineer; but by trying hard enough all difficulties could be surmounted for the axioms existed and the engine was perfect. But mathematicians were destined not to remain long in this comfortable position. The critical demon is a restless and relentless demon; and, having brought them thus far, it soon drove them far beyond. It was discovered that an axiom of a given set could be replaced by its contradictory and that the consequences of the new set stood all the experiential tests of truth just as well as did the consequences of the old set, that is, perfectly. Thus belief in the self-evidence of axioms received a fatal blow. For why regard a proposition self-evident when its contradictory would work just as well? But if we do not know that our axioms are true, what about their consequences? Logic gives us these, but as to their being true or false, it is indifferent and silent.

Thus mathematics has acquired a certain modesty. The critical mathematician has abandoned the search for truth. He no longer flatters himself that his propositions are or can be known to him or to any other human being to be true; and he contents himself with aiming at the correct, or the consistent. The distinction is not annulled nor even blurred by the reflection that consistency contains immanently a kind of truth. He is not absolutely certain, but he believes profoundly that it is possible to find various sets of a few propositions each such that the propositions of each set are compatible, that the propositions of such a set imply other propositions, and that the latter can be deduced from the former with certainty. That is to say, he believes that there are systems of coherent or consistent propositions, and he regards it his business to discover such systems. Any such system is a branch of mathematics. Any branch contains two sets of ideas (as subject matter, a third set of ideas are used but are not part of the subject matter) and two sets of propositions (as subject matter, a third set being used without being part of the subject): a set of ideas that are adopted without definition and a set that are defined in terms of the others; a set of propositions adopted without proof and called assumptions or principles or postulates or axioms (but not as true or as self-evident) and a set deduced from the former. A system of postulates for a given branch of mathematics—a variety of systems may be found for a same branch—is often called the foundation of that branch. And that is what the layman should think when, as occasionally happens,

he meets an allusion to the foundation of the theory of the real variable, or to the foundation of Euclidean geometry or of projective geometry or of *Mengenlehre* or of some other branch of mathematics. The founding, in the sense indicated, of various distinct branches of mathematics is one of the great outcomes of a century of critical activity in the science. It has engaged and still engages the best efforts of men of genius and men of talent. Such activity is commonly described as fundamental. It is very important, but fundamental in a strict sense it is not. For one no sooner examines the foundations that have been found for various mathematical branches and thereby as well as otherwise gains a deep conviction that these branches are constituents of something different from any one of them and different from the mere sum or collection of all of them than the question supervenes whether it may not be possible to discover a foundation for mathematics itself such that the above-indicated branch foundations would be seen to be, not fundamental to the science itself, but a genuine part of the superstructure. That question and the attempt to answer it are fundamental strictly. The question was forced upon mathematicians not only by developments within the traditional field of mathematics, but also independently from developments in a field long regarded as alien to mathematics, namely, the field of symbolic logic. The emancipation of logic from the yoke of Aristotle very much resembles the emancipation of geometry from the bondage of Euclid; and, by its subsequent growth and diversification, logic, less abundantly perhaps but not less certainly than geometry, has illustrated the blessings of freedom. When modern logic began to learn from such a man as Leibniz (who with the most magnificent expectations devoted much of his life to researches in the subject) the immense advantage of the systematic use of symbols, it soon appeared that logic could state many of its propositions in symbolic form, that it could prove some of these, and that the demonstration could be conducted and expressed in the language of symbols. Evidently such a logic looked like mathematics and acted like it. Why not call it mathematics? Evidently it differed from mathematics in neither spirit nor form. If it differed at all, it was in respect of content. But where was the decree that the content of mathematics should be restricted to this or that, as number or space? No one could find it. If traditional mathematics could state and prove propositions about number and space, about relations of numbers and of space configurations, about classes of numbers and of geometric entities, modern logic began to prove propositions about propositions, relations and classes, regardless of whether such propositions, relations and classes have to do with number and space or any other specific kind of subject. At the same time what was admittedly mathematics was by virtue of its own inner developments transcending its traditional limitations to number and space. The situation was unmistakable: traditional mathematics began to look like a genuine part of logic and no longer like a separate something to which another thing called logic applied. And so modern logicians by their own researches were forced to ask a question, which under a thin disguise is essentially the same as that propounded by the bolder ones among the critical mathematicians, namely, is it not possible to discover for logic a foundation that will at the same time serve as a foundation for mathematics as a whole and thus render unnecessary (and strictly impossible) separate foundations for separate mathematical branches?

It is to answer that great question that Messrs. Whitehead and Russell have written "Principia Mathematica"—a work consisting of three large volumes, the first being in hand, the second and third soon to appear—and the answer is affirmative. The thesis is: it is possible to discover a small number of ideas (to be called primitive ideas) such that all the other ideas in logic (including mathematics) shall be definable in terms of them, and a small number of propositions (to be called primitive propositions) such that all other propositions in logic (including mathematics) can be demonstrated by means of

them. Of course, not all ideas can be defined —some must be assumed as a working stock— and those called primitive are so called merely because they are taken without definition; similarly for propositions, not all can be proved, and those called primitive are so called because they are assumed. It is not contended by the authors (as it was by Leibniz) that there exist ideas and propositions that are absolutely primitive in a metaphysical sense or in the nature of things; nor do they contend that but one sufficient set of primitives (in their sense of the term) can be discovered. In view of the immeasurable wealth of ideas and propositions that enter logic and mathematics, the authors' thesis is very imposing; and their work borrows some of its impressiveness from the magnificence of the undertaking. It is important to observe that the thesis is not a thesis of logic or of mathematics, but is a thesis about logic and mathematics. It can not be proved syllogistically; the only available method is that by which one proves that one can jump through a hoop, namely, by actually jumping through it. If the thesis be true, the only way to establish it as such is to produce the required primitives and then to show their adequacy by actually erecting upon them as a basis the superstructure of logic (and mathematics) to such a point of development that any competent judge of such architecture will say: "Enough! I am convinced. You have proved your thesis by actually performing the deed that the thesis asserts to be possible."

And such is the method the authors have employed. The labor involved—or shall we call it austere and exalted play?—was immense. They had predecessors, including themselves. Among their earlier works Russell's "Principles of Mathematics" and Whitehead's "Universal Algebra" are known to many. The related works of their predecessors and contemporaries, modern critical mathematicians and modern logicians, Weierstrass, Cantor, Boole, Peano, Schröder, Peirce and many others, including their own former selves, had to be digested, assimilated and transcended. All this was done, in the course of more than a score of years; and the work before us is a noble monument to the authors' persistence, energy, acumen and idealism. A people capable of such a work is neither crawling on its belly nor completely saturated with commercialism nor wholly philistine. There are preliminary explanations in ordinary language and summaries and other explanations are given in ordinary language here and there throughout the book, but the work proper is all in symbolic form. Theoretically the use of symbols is not necessary. A sufficiently powerful god could have dispensed with them, and, unless he were a divine spendthrift, he would have done so, except perhaps for the reason that whatever is feasible should be done at least once in order to complete the possible history of the world. But whilst the employment of symbols is theoretically dispensable, it is, for man, practically indispensable. Many of the results in the work before us could not have been found without the help of symbols, and even if they could have been thus found, their expression in ordinary speech, besides being often unintelligible, owing to complexity and involution, would have required at least fifteen large volumes instead of three. Fortunately the symbology is both interesting and fairly easy to master. The difficulty inheres in the subject itself.

The initial chapter, devoted to preliminary explanations that any one capable of nice thinking may read with pleasure and profit, is followed by a chapter of 30 pages dealing with "the theory of logical types." Mr. Russell has dealt with the same matter in volume 30 of the *American Journal of Mathematics* (1908). One may or may not judge the theory to be sound or adequate or necessary and yet not fail to find in the chapter setting it forth both an excellent example of analytic and constructive thinking and a worthy model of exposition. The theory, which, however, is recommended by other considerations, originated in a desire to exclude from logic automatically by means of its principles what are called illegitimate totalities and therewith a subtle variety of contradiction and vicious circle fallacy that, owing their presence to the

non-exclusion of such totalities, have always infected logic and justified skepticism as to the ultimate soundness of all discourse, however seemingly rigorous. (Such theoretic skepticism may persist anyhow, on other grounds.) Perhaps the most obvious example of an illegitimate totality is the so-called class of all classes. Its illegitimacy may be shown as follows. If A is a class (say that of men) and E is a member of it, we say, E is an A. Now let W be the class of all classes such that no one of them is a member of itself. Then, whatever class x may be, to say that x is a W is equivalent to saying that x is not an x, and hence to say that W is a W is equivalent to saying that W is not a W! Such illegitimate totalities (and the fallacies they breed) are in general exceedingly sly, insinuating themselves under an endless variety of most specious disguises, and that, not only in the theory of classes but also in connection with every species of logical subject-matter, as propositions, relations and propositional functions. As the propositional function—any expression containing a real (as distinguished from an apparent) variable and yielding either non-sense or else a proposition whenever the variable is replaced by a constant term—is the basis of our authors' work, their theory of logical types is fundamentally a theory of types of propositional functions. It can not be set forth here nor in fewer pages than the authors have devoted to it. Suffice it to say that the theory presents propositional functions as constituting a summitless hierarchy of types such that the functions of a given type make up a legitimate totality; and that, in the light of the theory, truth and falsehood present themselves each in the form of a systematic ambiguity, the quality of being true (or false) admitting of distinctions in respect of order, level above level, without a summit. When Epimenides, the Cretan, says that all statements of Cretans are false, and you reply that then his statement is false, the significance of "false" here presents two orders or levels; and logic must by its machinery automatically prevent the possibility of confusing them.

Next follows a chapter of 20 pages, which all philosophers, logicians and grammarians ought to study, a chapter treating of Incomplete Symbols wherein by ingenious analysis it is shown that the ubiquitous expressions of the form "the so and so" (the "the" being singular, as "the author of Waverley," "the sine of a," "the Athenian who drank hemlock," etc.) do not of themselves denote anything, though they have contextual significance essential to discourse, essential in particular to the significance of identity, which, in the world of discourse, takes the form of "a is the so and so" and not the form of the triviality, a is a.

After the introduction of 88 pages, we reach the work proper (so far as it is contained in the present volume), namely, Part I.: Mathematical Logic. Here enunciation of primitives is followed by series after series of theorems and demonstrations, marching through 578 pages, all matter being clad in symbolic garb, except that the continuity is interrupted here and there by summaries and explanations in ordinary language. Logic it is called and logic it is, the logic of propositions and functions and classes and relations, by far the greatest (not merely the biggest) logic that our planet has produced, so much that is new in matter and in manner; but it is also mathematics, a prolegomena to the science, yet itself mathematics in the most genuine sense, differing from other parts of the science only in the respects that it surpasses these in fundamentality, generality and precision, and lacks traditionality. Few will read it, but all will feel its effect, for behind it is the urgence and push of a magnificent past: two thousand five hundred years of record and yet longer tradition of human endeavor to think aright.

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A LETTER OF LAMARCK

LETTERS of Lamarck are not often found. M. Landrieux, who has recently published a life of Lamarck, states that "one can count the number of his letters which have come

down to us,"[1] and that "even his autographs are exceedingly rare."

It is of interest, therefore, that another letter in the hand of Lamarck has been discovered in Paris (it is now in my possession), and as it contains several data regarding his life, it may be worthy of publication. It reads as follows:

PARIS ce 16 floréal au 5 de la Rep.

Lamarck professeur au Museum d'hist. naturelle

Au Citoyen Cyalis Lavaux Directeure de la 1ere division des domaines

Citoyen

ayant reçu de Songeons, département de l'oise, l'avis que la vente des biens que j'ai Soumissionés etoit Suspendue et qu'on avoit méconnue l'autorité du Ministre des finances, j'au eû l'honneur de vous faire passer une lettre par laquelle je me Suis plaint de la Conduite du département à cet egard. j'apprends en ce moment que la personne qui m'a fait passer cet avis m'a trompé, et que le département de l'oise n'a encore pris aucun arreté qui me fut prejudiciable. je me hâte de vous en instruire pour vous prier de ne donner aucune Suite à la lettre que j'ai eû l'honneur de vous faire passer.

quand le Ministre de l'interieur aura fait passer à Son Collegue le Ministre des finances les Conditions de la vente de ma Collection, je me recommande à votre bienveillance que vous avez deja bien voulu me promettre, et pour laquelle je vous prie d'agréer ma vive reconnaissance et mes Salutations fraternelles.

LAMARCK

It will be seen from the above details that Lamarck, like many members of his class, was troubled in matters of property during the upheaval of the revolution. He had maintained his post, poor as it was, in the Jardin des Plantes in 1790, and had made a strong plea for a reorganization of this institution under the republican régime: he had even (1793) gained the day and during the most democratic epoch, he was distinguished as one of the first professors placed in charge of the collections of the Jardin des Plantes. The present letter shows, none the less, that in 1794 Lamarck was concerned about his property at Songeons. The authorities had not, however, as he later ascertained, confiscated it, and ordered its sale. Still, times were bad and he was probably in financial straits, since he took the opportunity in the same letter to refer to the matter of the sale of his (private) collection, and to "pull a wire" more or less insistently.

BASHFORD DEAN

[1] "Lamarck le Fondateur du Transformism," 1909, p. 105.

SPECIAL ARTICLES

THE APPARENT ANTAGONISM BETWEEN ELECTROLYTES AND NONCONDUCTORS

1. In a publication contained in SCIENCE, Vol. XXXIV., No. 887, pp. 928, Sumner makes the following statement: "Loeb's assertion that 'salts alone have such antagonistic effects' certainly does not apply to adult fishes. I need only call attention to the fact that cane-sugar solutions of certain strengths were found by me to very clearly defer the fatal action of the copper salts, both upon *Fundulus heteroclitus* and upon certain fresh-water species."

Thirteen years ago I pointed out the fundamental difference between the influence of electrolytes and nonelectrolytes upon life phenomena and in later publications called attention to the fact that this difference indicated an interaction between the electrolytes and colloids, especially the proteins, of the cells, which did not exist between nonelectrolytes and the same colloids of the cells. The further development of colloid chemistry and biology has shown that this conclusion was correct and fruitful. The fact that the toxic action of electrolytes upon the cells can be antagonized by electrolytes only is a special case of this more general rule. In 1902 Gies and I published an apparent exception to this rule, namely, that the toxic action of $ZnSO_4$ upon *Fundulus* eggs could be inhibited through the addition of cane-sugar; but we pointed out that in this case there is no antagonistic action between $ZnSO_4$ and cane-sugar in regard to the colloids of the egg (or membrane) but a chemical reaction between $ZnSO_4$ and cane-sugar which leads to the formation of zinc saccharate, and consequently to a diminution of the Zn ions in solu-

tion. Five years later Sumner published his observations that the action of copper salt can be deferred through the addition of cane-sugar, which is of course similar to the observation by Gies and myself. In the case of the antagonization of $ZnSO_4$ by another electrolyte we are, however, dealing with the action of both electrolytes on the same colloid.

2. Sumner states also that distilled and fresh water are toxic for *Fundulus* and that there exists an antagonism between distilled water and salts for these fish. The fact that a number of *Fundulus* can live a long time (if not indefinitely) in distilled water and that these fish, if they become landlocked, can live indefinitely in fresh water indicates that the distilled or fresh water are not in themselves toxic for these animals but that the toxic effect occasionally (but not always) observed is due to an inconstant or quantitatively varying constituent of the water. This constituent may be a parasite, or it may be a substance given off by the fish itself, *e. g.*, CO_2. Wasteneys and I have recently found that CO_2 may produce the same changes on the skin and the gills of the fish as those produced by mineral acids; and that, as in the latter case, the etching effects of the CO_2 may be counteracted through the addition of a neutral salt. The beneficial effect of the addition of some salt to the fresh or the distilled water, therefore, indicates that the salt either kills certain parasites contained or developed in the distilled water, or antagonizes the toxic effects of some electrolyte, *e. g.*, carbonic acid, if its concentration exceeds a certain limit, as it possibly did in some or all of Sumner's experiments.

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THE PERMEABILITY OF PROTOPLASM TO IONS AND THE THEORY OF ANTAGONISM

EVIDENCE was recently presented which showed[1] that a great variety of salts readily enter living cells and that antagonism between salts may be due to the fact that they mutually hinder or prevent each other from penetrating the protoplasm.

In these experiments plasmolysis was the criterion of penetration. Plasmolysis shows which salts enter and how rapidly, but does not indicate whether it is the ions or only undissociated molecules which penetrate the cell. To decide this question experiments were performed to test the electrical conductivity of living tissues in various solutions. The results agree in showing most conclusively that ions readily penetrate living protoplasm and that many ions which penetrate quite rapidly in pure solutions may be hindered or prevented from going in by the addition of small amounts of $CaCl_2$ or other salts.

To obtain reliable results in conductivity experiments material should be used which is not injured by weak currents or by other experimental conditions. It is desirable that the amount of space between the cells be constant so that the current which passes between the cells may be a constant fraction (as small as possible) of that which actually traverses the living protoplasm. The current should pass through a large number of thin sheets of living tissue, separated by thin films of solution. The penetration of various ions may then be studied by merely changing the solution. If the material is in thin sheets the ions are forced by the alternating current to pass in and out of a great extent of protoplasmic surface; this is of great importance, since the larger the surface the more reliable the measurement. The sheets of living tissue should be sufficiently rigid to permit manipulation and to endure without injury pressure sufficient to pack them firmly together so that the films of solution which separate them may be as thin as possible.

All these conditions are admirably fulfilled by the common kelps of the Atlantic coast (species of *Laminaria*). This material was accordingly used throughout the investigations.

Disks about 13 mm. in diameter were cut from the fronds by means of a cork-borer (the average thickness of the frond was about 0.5 mm.). From 100 to 200 of these disks

[1] SCIENCE, N. S., 34: 187, 1911.

were packed together (like a roll of coins) into a solid cylinder from 50 mm. to 100 mm. long. They were firmly held in place by glass rods arranged to make a hollow cylinder which closely fitted over the outside of the solid cylinder of tissue. Spaces between the rods allowed free access of the solution to the living tissue. At each end of the cylinder of tissue was placed a block of hard rubber containing a platinum electrode covered with platinum black; by means of a screw these blocks could be pressed with considerable force against the opposite ends of the cylinder of living tissue. The only substances which came into contact with the solution were hard rubber, glass, the electrodes and the living tissue. Details of construction will be given in a subsequent paper.

The current after leaving the electrodes traversed the solution for a very short distance and then passed directly into the living tissue. The same solution which bathed the electrodes was also present between the disks of living tissue in the form of thin films. The surface in and out of which ions were forced by the current amounted to from 26,500 to 53,000 sq. mm.

The measurements were made in the usual manner by means of a Wheatstone bridge. The solutions were brought to the same temperature before measuring except that in the case of small deviations the proper correction was made.

The usual method of procedure was to place the cylinder of disks in the solution, clamp the electrode carriers firmly against both ends of it, lift it out of the solution and read the resistance as soon as the superfluous liquid had drained from the tissue.

A preliminary series of experiments showed that the material remained to an extraordinary degree uninjured by the action of the currents employed as well as by the additional treatment involved in the experiments. In the first experiments the material was usually left immersed in sea water in the apparatus for 24 hours. During this period 12 readings were taken (the current passing for about two minutes each time) and the disks were 12 times taken out and then replaced in the apparatus. At the end of the 24 hours the resistance to the current remained unchanged (if injury had occurred the resistance would have been diminished) and there was no indication either macroscopic or microscopic that the cells were injured.

If the plasma membrane and the cell wall presented no obstacle to the passage of ions we should expect the resistance of a cylinder of living tissue to be practically that of a similar cylinder full of sea water. It was found that a cylinder of living tissue had a resistance of 1,100 ohms (all the figures given in this paper refer to readings taken between 18° C. and 18.2° C.) while that of a cylinder of sea water of equal size was 320 ohms. To ascertain whether this excess of resistance was due to living protoplasm or to cell walls the protoplasm was killed by adding sufficient formalin to the sea water to make a 2 per cent. solution. In other experiments the disks were killed by careful drying. In all cases the resistance after killing fell to about 320 ohms. These experiments demonstrated in the clearest manner that the ions penetrated very much less rapidly into living cells than into dead protoplasm or into cell walls.

Experiments were then made to determine the rate of penetration of various ions. As the treatment was the same in all cases it will suffice to describe a typical experiment dealing with NaCl and $CaCl_2$.

The material was first tested in sea water and found to have a resistance of 1,100 ohms. After remaining four hours in sea water the resistance was unchanged. The material was then transferred to NaCl .52 *M* which had the same temperature as the sea water and the same conductivity (as determined by numerous careful tests). The electrode carriers were unclamped and moved apart. Each disk was then seized in turn by the forceps and moved back and forth in the solution so as to wash out the sea water and replace it by the solution of pure NaCl. This was repeated several times. It was thus possible to rinse each disk thoroughly in the solution without removing it from the apparatus or changing

its position in the series of disks which composed the cylinder. The mere act of rinsing the disks in this way and then reclamping the electrodes made only slight changes in the reading.

After remaining five minutes in NaCl .52 *M* the resistance had dropped to 1,000 ohms; after ten minutes to 890 ohms; after fifteen minutes to 780 ohms; after sixty minutes to 420 ohms. It continued to fall steadily until it reached 320 ohms, at which point it remained stationary; it then had practically the conductivity of sea water. On replacing in sea water it did not recover any of its resistance, even after standing for several days. It should be noted that the solution of NaCl employed is nearly isotonic with sea water and that none of the observed effects could be due to osmotic action.

Further experiments showed that if the material was removed from NaCl solution and placed in sea water as soon as its resistance had fallen about one hundred ohms below the original resistance it quickly regained its original resistance and remained unchanged for a long time.

It is therefore evident that pure NaCl produces a very rapid decrease in resistance which, up to a certain point, is reversible.

A very striking contrast is obtained by placing living tissue in a solution of $CaCl_2$ having the same conductivity as sea water. The resistance then rises rapidly to a maximum (very often in the first fifteen minutes from 1,100 ohms to 1,750 ohms) and remains practically stationary for some hours. After this it slowly sinks and finally reaches about 320 ohms, which is the resistance of an equal amount of sea water. If, however, it be returned to sea water shortly after it has reached its maximum it soon regains its original resistance and remains for a long time (in sea water) practically unchanged. The rise in resistance caused by $CaCl_2$ is in no way due to its action on the cell walls, for dead tissue shows no rise.

It is therefore evident that $CaCl_2$ produces a very rapid *increase* in resistance, which is reversible.

What is the effect of combining NaCl and $CaCl_2$ in the proportions in which they exist in sea water? This question has great theoretical and practical interest in view of the fact that $CaCl_2$ is known to antagonize the toxic action of NaCl in the most striking way. To answer this question the following experiment was performed. To 1,000 c.c. NaCl 1 *M* there was added 15 c.c. $CaCl_2$ 1 *M;* the mixture was then diluted until it had the same conductivity as sea water. On placing living tissue in this mixture it neither gained nor lost in resistance and even after twenty-four hours had the same resistance as at the start.

It is therefore evident that the entrance of the ions of NaCl is greatly hindered by the presence of very small amounts of $CaCl_2$ and that this may explain the antagonistic action of $CaCl_2$ on NaCl.

Further experiments showed that such salts as KCl, $MgCl_2$, CsCl, RbCl, LiCl, NH_4Cl, NaBr, NaI, $NaNo_3$, Na_2So_4 and Na-acetate act in general like NaCl (though with different degrees of rapidity) while $BaCl_2$ and $SrCl_2$ act like $CaCl_2$.

It might be supposed that some of these effects are due to expansion or contraction of the cells under the influence of the salts, but microscopic observation showed that this was not the case except only that when a cell is injured by the salt a contraction (which I have elsewhere called false plasmolysis) may take place. But as the fall in resistance is already great before any such contraction begins and as the contraction is in any case too small to account for more than a small per cent. of the decrease in resistance it may be regarded as at best a secondary factor which is absent until the resistance has reached a low point and which is almost negligible beyond that point.

It might be supposed that the change in resistance is due to causes which operate in the interior of the cell rather than in the plasma membrane, but this is opposed to a variety of evidence which can not be discussed here.

Two hypotheses may be formed regarding

the increase of resistance which is observed when the tissue is transferred from $NaCl + CaCl_2$ to pure $CaCl_2$ of the same conductivity. On one hypothesis the plasma membrane would retain its normal properties after the transfer but would show increased resistance because it is normally less permeable to the ions of $CaCl_2$ than to the ions of NaCl.

On the other hypothesis the plasma membrane would suffer a change in its properties as the result of the transfer. The facts strongly favor this hypothesis. I will mention only a few. Visible changes in the outer layer of the protoplasm are produced by $CaCl_2$ (and many other substances) and this makes it probable that the plasma membrane suffers change. Alum, which is known to alter the properties of many colloids (*e. g.*, in tanning), when added in solid form to the sea water greatly increases the resistance of the protoplasm although it greatly decreases the resistance of the sea water. In this case the only explanation is that the permeability of the plasma membrane is altered. On the other hand it is clear that the large number of substances which produce irreversible decrease of resistance must also alter the plasma membrane.

It seems probable therefore that a great variety of substances alter the plasma membrane so as to increase or decrease its permeability.

It may be pointed out that these results are precisely what should be expected if the antagonistic action of salts is due, as Loeb has suggested, to the fact that they hinder each other from penetrating the protoplasm. It is quite clear from the experiments that $CaCl_2$, $SrCl_2$ and $BaCl_2$ in small amounts are able to hinder very greatly the entrance of the ions of NaCl. The mechanism of this action is not fully understood, but I may state that $CaCl_2$, $BaCl_2$ and $SrCl_2$ bring about visible changes in the plasma membrane which are entirely different from those produced by such salts as NaCl. It is hoped that a further study of these visible changes may throw light on this question.

Previous experiments on plasmolysis have shown essentially similar phenomena and the complete confirmation of the results of one method by those of the other form the most striking proof possible of the facts outlined above.

It may be asked how merely delaying the entrance of a salt can protect the protoplasm against its toxic action. In this connection it may be pertinent to recall the familiar phenomenon of colloid chemistry that a salt which produces marked effects when added suddenly may produce little or no effect when added slowly. It should be noted that there is good evidence to show that the NaCl does not enter the cell alone but is accompanied by $CaCl_2$. It is possible that these salts may wholly prevent each other from penetrating internal membranes (*e. g.*, the nuclear membrane) which are of importance in this connection.

The chief conclusions are as follows:

1. Quantitative studies of permeability may be made by a simple and accurate method.
2. Slight changes in the rate of penetration may be observed and accurately measured at very brief intervals.
3. A great variety of anions and kations readily penetrate living protoplasm.
4. Inasmuch as these ions are insoluble in lipoid it would appear that Overton's theory of permeability can not be correct.
5. The plasma membrane is readily altered by a variety of substances in a fashion which is easily understood on the hypothesis of a colloid (probably proteid) plasma membrane but which can not be explained on the hypothesis that the plasma membrane is a lipoid.
6. The antagonistic action of salts is largely or entirely due to the fact that they hinder or prevent one another from entering the protoplasm.

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NOTES ON THE DISTRIBUTION OF THE SOUTHEASTERN SALAMANDERS (GEOMYS TUZA AND ALLIES)

A CHARACTERISTIC feature of many parts of the pine forests of the coastal plain of Georgia,

Alabama and Florida is the common occurrence of small mounds of sand a foot or more in diameter and a few inches high, scattered irregularly over the surface or in more or less evident lines, and usually several feet apart. A traveler from farther northeast, seeing these mounds for the first time, might easily imagine that some one had been driving along with a wagon-load of sand and dumping it out in large shovelfuls. They are seen best in winter and spring, partly because the surrounding vegetation is less conspicuous then, and partly for other reasons.

These diminutive mounds cover the outlets of the burrows of a subterranean rodent, *Geomys Tuza*, which is known throughout its range as the "salamander."[1] (Zoologists have divided the original *G. Tuza* into some half-dozen species and subspecies, but there are no wider gaps between the ranges of the different forms than there are between some colonies of the same species, and they all have essentially the same habits and habitat, so that for the purposes of this discussion it will be most convenient to treat the group as a unit.) The animal feeds on roots, travels entirely underground, as far as known, and very rarely shows itself, in the daytime at least.

While working for the Florida State Geological Survey in 1908–1910 I had occasion to visit every county and to travel on nearly every railroad in that state; and on railroad journeys I usually had nothing better to do than look out of the car-window and make notes on the topography, vegetation and other geographical features. The salamander hills, which certainly constitute one of the topographic features, even if a very insignificant one, thus received their share of attention, and in that way I have accumulated records of hundreds of precise localities for this elusive animal. On plotting these on a map recently some interesting correlations between them and certain other geographical features became evident. In previous years I had visited every county in Georgia and Alabama in which *Geomys* is known, but my notes on its distribution in these two states are much less complete than they are for Florida.

In both Georgia and Alabama the salamander ranges all the way across the coastal plain up to the fall-line, in about latitude 33° 15′, but one can travel many miles without seeing any evidences of it, and it is much less abundant in those states and in west Florida than it is in peninsular Florida. The Biological Survey of the U. S. Department of Agriculture has a record[2] of just one station for it outside of the coastal plain, namely, near Chipley, Georgia.[3] In Alabama the only known stations for it north of the latitude of Montgomery seem to be around Kingston, in Autauga County; in the northeastern corner of Hale County; on high pine hills near Lock 14 on the Warrior River, and between Brookwood and Searles. At both of the last-named stations, which are in the upper (northeastern) part of Tuscaloosa County, the salamander hills are found over Carboniferous rocks, but always where there is a thin layer of some unconsolidated coastal plain deposit, presumably the Lafayette, on the surface.

In Florida salamander hills can be seen in abundance at frequent intervals all the way down to a point between Nocatee and Fort Ogden in DeSoto County, about latitude 27° 10′, which is about fifty miles farther south than the southernmost station for *Geomys* mentioned by Outram Bangs in his interesting paper on the land mammals of

[1] Dr. C. Hart Merriam in his elaborate monograph of *Geomys* and related genera ("N. Am. Fauna No. 8," p. 112, 1895) characterized this common name as "singularly inappropriate and misleading." But contrary opinions as to its appropriateness have been expressed by Goode (Powell's Report on Colorado River, p. 281, 1875) and Bangs (*Proc. Bost. Soc. Nat. Hist.*, 28, p. 175, 1898); and it is no more misleading than the names cypress, cedar, sycamore and poplar, applied to very different trees in this country from what they are in the old world.

[2] Unpublished, but communicated to me by Mr. A. H. Howell.

[3] There happens to be also a Chipley in Florida, a more important place than the one in Georgia, and it is barely possible that the specimen in question came from Florida and was ascribed to Georgia by a slip of the pen.

peninsular Florida.[4] I have seen them in every county in that state except Franklin, Manatee, Lee, Osceola, Brevard, St. Lucie, Palm Beach, Dade and Monroe, all of which were included by Dr. Eugene A. Smith, in his classical paper on the geography of Florida,[5] in what he called the "pitch-pine, treeless and alluvial region." These counties are all in south Florida with the exception of Franklin, which is in middle and west Florida, over 200 miles from the rest. Dr. Smith admitted that this was not a very homogeneous area, but he grouped these counties together for convenience because they produced almost no cotton (only two bales being reported from that whole area in 1880). The distribution of *Geomys* now furnishes an additional character for distinguishing these counties from the remainder of the state; for I have seen salamander hills in every county included by Dr. Smith in his other two regions, namely, the "long-leaf pine region," and the "oak, hickory and pine upland region." (The counties in Florida are of course more numerous now than they were at the time of the Tenth Census, but it so happens that that does not affect the truth of this statement.)

About a year ago, in a report on the peat deposits of Florida,[6] the writer divided the state provisionally into fourteen geographical divisions. Of these the lime-sink region and the lake region, near the axis of the peninsula, seem to be the headquarters of the salamander. The animal is not known at all in the East Coast strip or the coast region of West Florida, both of which consist chiefly of modern (active) dunes next to the ocean and ancient (fixed) dunes a little farther back. It occurs in varying degrees of abundance in the remaining divisions, except those south of the latitude of Lake Okeechobee.

[4] *Proc. Bost. Soc. Nat. Hist.*, 28, p. 176, 1898.

[5] Tenth Census U. S., 6, pp. 175–257, 1884. This monograph, like those on other southeastern states in the same volume, bears the entirely too modest designation of a report on cotton production.

[6] Ann. Rep. Fla. Geol. Surv., 3, pp. 201–375, pls. 16–28, January, 1911.

The range of the southeastern salamander[7] seems to terminate abruptly on the east at the Savannah River, and on the west at the Warrior and the streams which under two or three other names connect that river with the Gulf of Mexico. From all accounts it appears that the various subspecies (or species as some regard them) of this group, which are the only representatives of the genus east of the Wabash River and its continuations, nowhere occupy the same territory, but are separated by rivers, which must be almost impassable barriers to an animal which spends its life underground and has no use for water. Indeed it is difficult to imagine how such an animal could ever have crossed any of the large rivers which extend all the way across the coastal plain; but at some time in the past the ancestors of the present salamanders must have crossed at least three such rivers, namely, the Altamaha, Apalachicola and Alabama. (The crossing of the Mississippi and Ohio rivers by members of the genus must have taken place at a much more remote period, judging from the much greater geographical and phylogenetic gap between the species on opposite sides of these rivers.) Mr. Bangs, in his paper already cited, expressed the opinion that *G. Floridanus* is separated from the other forms by the St. Mary's and Suwannee rivers, with Okefinokee Swamp connecting their headwaters. If this is true then the salamanders of middle Florida (*i. e.*, that part of the state between the Suwannee and Apalachicola rivers) must be typical *G. Tuza,* the same as in Georgia. However that may be, salamander hills of exactly identical appearance can be found within a mile or two of each other on opposite sides of the Suwannee, which has almost no swamps where it passes through the lime-sink region.

So much for the areal distribution of our salamander. Some interesting correlations

[7] According to Dr. Merriam's monograph previously cited, *Geomys Tuza* and its near relatives are confined to the three states already named, but the same common name is also applied to another species which inhabits Arkansas and Louisiana.

between it and certain soils and other environmental factors can now be made. Its hills are almost always in dry sandy loam,[8] presumably of Pliocene or Pleistocene age. It avoids on the one hand the fertile limestone and clay soils which characterize some parts of the coastal plain, and on the other the hopelessly sterile sands of the shifting dunes along the coast and the "scrub" of the Florida peninsula. It is most abundant in regions where according to the statistical maps in the fifth and sixth volumes of the Tenth Census less than one acre to the square mile was cultivated in cotton in 1880; but in soils which at the present stage of the economic development of the southeastern United States are being appropriated by farmers most rapidly. Being confined to dry soil, it is absent from land which is too damp for cultivation by ordinary methods (as well as that which is too sterile or too rocky). But I have never heard any complaints about its interfering with agricultural operations.

The southeastern salamander seems to be invariably associated with the long-leaf pine (*Pinus palustris*), and it may derive part of its food from the roots of that useful tree. The only known station in the Piedmont region of Georgia, mentioned above, is probably not right in the city of Chipley, but very likely on the Pine Mountains near by, where the rocks and soil are pretty sandy, and long-leaf pine abounds.[9] The range of our animal is by no means coextensive with that of the long-leaf pine, though, for the tree ranges from Virginia to Texas, as well as considerably farther inland in Georgia and Alabama and a little farther south in Florida than the salamander does. Two other trees usually found in the vicinity of salamander hills, and having more nearly the same range, are two scrubby oaks, *Quercus Catesbaei* and *Q. cinerea*. Mr. Bangs (*op. cit.*, p. 180) states that Cumberland Island, Georgia, is the only one of the sea-islands (which fringe the coast from about Charleston to Jacksonville) on which a *Geomys* occurs. It is also, to the best of my knowledge and belief, the only one which has *Pinus palustris* and *Quercus Catesbæi* on it; and its geological history must have been somewhat different from that of the others.

Lastly there are some interesting relations between the salamander and forest fires, as was noticed briefly long ago in the papers cited in the first footnote.[10] Every long-leaf pine forest, without exception, is periodically swept by fire, which burns off the dead herbage and keeps down the underbrush, but does no harm to sound pine trees after they get beyond a certain age. (In prehistoric times these fires, presumably set by lightning, probably did not visit any one spot oftener than once in four or five years, on the average; but now so many fires are set accidentally or purposely by man that few of these forests escape fire longer than a year or two at a time.)[11] The dunes and scrub of Florida, mentioned above, as well as the other extreme, the rich hammocks, have so little herbage that fires are very rare, and in such places there are neither long-leaf pines nor salamanders.

Fires in the southern pine woods are most frequent in late winter and early spring, and the salamanders seem to be most active just about that time. The locality near Lock 14 on the Warrior River, when first discovered on April 15, 1911, had evidently been burned over a few days or weeks previously, and the salamander hills there looked pretty fresh. What is still more interesting, none could be found in precisely similar areas near by

[8] At the northernmost Alabama stations there is a considerable admixture of gravel in the soil, and it is possible that if specimens could be obtained from these somewhat isolated localities they might be found to differ perceptibly in some characters from the only form at present known in that state, *G. Tuza Mobilensis.*

[9] See *Bull. Torrey Bot. Club*, 36, pp. 585–586, 1909.

[10] The same relation was noticed still earlier by Sir Charles Lyell, the English geologist, in Screven County, Georgia, in the winter of 1841–42. ("Travels in North America," Vol. I., p. 161, 1845.)

[11] In this connection, see *Bull. Torrey Bot. Club*, 38, p. 522, 1911.

which had not been burned recently. The locality was revisited in May and June, during which time there were no more fires, and the salamander hills were gradually settling down and disappearing, no new ones having been made in the interval, apparently.[12] It would be very interesting to know if the related species of the middle northwest, *G. bursarius*,[13] shows a similar reaction to prairie fires.

ROLAND M. HARPER

[12] In June Mr. A. H. Howell set traps in several of these hills, but without catching anything, which seems to indicate that the animals were not working near the surface at that time.

[13] Since Dr. Merriam's monograph some additional notes on the habits of this species have been published by Mr. C. L. Webster in the *American Naturalist*, 31, pp. 114–120, 1897.

THE AMERICAN MATHEMATICAL SOCIETY

THE eighteenth winter meeting of the society was held at Columbia University on Wednesday and Thursday, December 27–28, 1911, the program occupying two sessions on each day. The total attendance was about seventy-five, including sixty-three members. President H. B. Fine occupied the chair, being relieved by ex-President H. S. White. The following new members were admitted: Professor Ida Barney, Rollins College; Professor Louis Brand, University of Cincinnati; Professor C. W. Cobb, Amherst College; Professor J. C. Fitterer, University of Wyoming; Mr. G. H. Graves, Columbia University; Dr. Solomon Lefschetz, University of Nebraska; Mr. G. H. Light, Purdue University; Mr. E. S. Palmer, Rutgers College; Professor E. R. Smith, Pennsylvania State College. Eight applications for membership were received. The total membership of the society is now 669, an increase of 27 during the past year.

On Wednesday evening forty-two of the members gathered at the annual dinner at the Murray Hill Hotel. These informal dinners have long been recognized as one of the most attractive features of the meetings.

The treasurer's report shows a balance of $8,723.89, or deducting outstanding bills, about $8,200, a slight increase for the year. The income from sales of publications was $1,513.66. The life membership fund now amounts to $4,137.17. The number of papers read at all meetings of the year was 180; the total attendance of members was 350. At the annual election 197 votes were cast. The society's library has increased to 3,840 volumes, beside some 500 unbound dissertations.

At the annual election, which closed on Thursday morning, the following officers and other members of the council were chosen: *Vice-presidents*, H. F. Blichfeldt and Henry Taber; *Secretary*, F. N. Cole; *Treasurer*, J. H. Tanner; *Librarian*, D. E. Smith; *Committee of Publication*, F. N. Cole, E. W. Brown and Virgil Snyder; *Members of the Council* (to serve until December, 1914), A. B. Coble, E. W. Davis, Oswald Veblen and E. B. Wilson.

The following papers were read at this meeting:

W. M. Smith: "A characterization of isogonal and equitangential trajectories."

C. L. E. Moore: "Surfaces in hyperspace which have a tangent line with three-point contact passing through each point."

J. E. Rowe: "How to find a set of invariants for any rational curve of odd order."

J. E. Rowe: "A covariant point of the R^4, and a special canonical form."

R. L. Moore: "On the sufficient conditions that an integral equation of the second kind shall have a continuous solution."

E. B. Wilson: "Some mathematical aspects of relativity."

Edward Kasner: "Families of surfaces related to an arbitrary deformation of space."

H. B. Phillips and C. L. E. Moore: "Algebra of plane projective geometry."

Anna L. Van Benschoten: "Products of quadric inversions and linear transformations in space" (preliminary report).

Arthur Ranum: "N-dimensional spreads generated by ∞^1 flats."

O. E. Glenn: "Generalizations of a theorem on reducible quantics, due to Eisenstein."

F. R. Sharpe: "Finite groups of birational transformations in the plane."

John Eiesland: "On a flat spread-sphere geometry in an odd dimensional space."

C. N. Moore: "The summability of the double Fourier series, with applications."

S. E. Slocum: "A general formula for torsional deflection."

G. A. Miller: "Groups which contain a given number of operators whose orders are powers of the same prime."

R. G. D. Richardson: "Theorems of oscillation for three self-adjoint linear differential equations of the second order with three parameters."

L. A. Howland: "Points of undulation of algebraic curves."

C. N. Haskins: "Note on certain selective integrals."

L. P. Eisenhart: "Ruled surfaces with isotropic generators."

J. W. Young: "On algebras defined by groups of transformations."

J. W. Young: "A generalization to 3-space and to n-space of the inversion geometry in a plane" (preliminary communication).

L. L. Silverman: "On absolute or unconditional summability."

L. L. Silverman: "Tests for Cesàro summability."

W. B. Fite: "Note on a collineation group in n variables."

L. P. Eisenhart: "Congruences of minimal lines in a four-space."

The Chicago Section of the society met at the University of Chicago on December 29–30. The next meeting of the society will be held at Columbia University on February 24.

F. N. Cole,
Secretary

THE AMERICAN PHILOSOPHICAL ASSOCIATION

The American Philosophical Association held its eleventh annual meeting at Harvard University, Cambridge, on December 27, 28 and 29, 1911. The meeting was marked by an unusually large attendance and by keen interest in the proceedings. A majority of the papers read centered around two main subjects selected for discussion, the one on the relation of object and consciousness in sense perception, the other on evolution. The discussion of the first subject was characterized by a departure from the method of previous meetings. This year both the papers read and the subsequent discussion were based on a carefully prepared committee report, in which both tentative definitions and an analysis of the problem were made. While this method did not result in an agreement of any great extent, it served to clarify the problem and to bring to a sharper focus the various issues involved in it. The committee which prepared this report was continued and given power to select the problem for next year, to formulate the issues involved in it, and to arrange for the discussion. Three papers and the presidential address were read on the subject of evolution, and these as well as the other papers read received vigorous discussion. The association empowered the executive committee to arrange, if possible, some future meetings with other societies in order that joint sessions may be held to discuss such questions as that of mechanism and vitalism.

E. G. Spaulding,
Secretary

THE AMERICAN ASSOCIATION OF ECONOMIC ENTOMOLOGISTS

The 24th annual meeting of the American Association of Economic Entomologists was held in the new National Museum, Washington, December 27–29. The meeting was called to order on Wednesday afternoon, December 27, and after the routine business of opening the session was disposed of the annual address entitled "The Relation of the Economic Entomologist to His Environment" was delivered by the president, F. L. Washburn, state entomologist of Minnesota.

On the following day papers were read at morning and afternoon sessions, and on Friday morning, at the close of several interesting papers, the final business of the session was transacted and the meeting adjourned.

The report of the secretary showed that the membership of the association had increased slightly during the year of 1911, and at the meeting over forty new members were admitted. This makes the total membership of the association, exclusive of foreign members, more than three hundred, and the interest shown in the association, especially by the younger members, augers well for its future success.

Among the more important business matters that were transacted was an arrangement whereby the official organ of the association, the *Journal of Economic Entomology,* will, in the future, be published by the association instead of by the publishing company, as heretofore. The present editorial board will serve for the coming year, with the exception of the business manager of the *Journal,* Professor E. D. Sanderson, Morgantown, West Virginia, who voluntarily retired. The management of the *Journal* has been placed in the hands of the secretary of the association.

The following officers were elected:

President—W. D. Hunter, Dallas, Texas.

First Vice-president—T. J. Headlee, Manhattan, Kansas.

Second Vice-president—R. A. Cooley, Bozeman, Montana.

Secretary—A. F. Burgess, Melrose Highlands, Mass.

A full account of the business transacted at the meeting together with the papers read with the discussions will be published in the *Journal of Economic Entomology,* the first number of which for the current year will be issued February 15.